A

# JOURNAL

OF

# NATURAL PHILOSOPHY,

*&c. &c. &c.*

# TABLE OF CONTENTS

## TO THE THIRTY-SIXTH VOLUME.

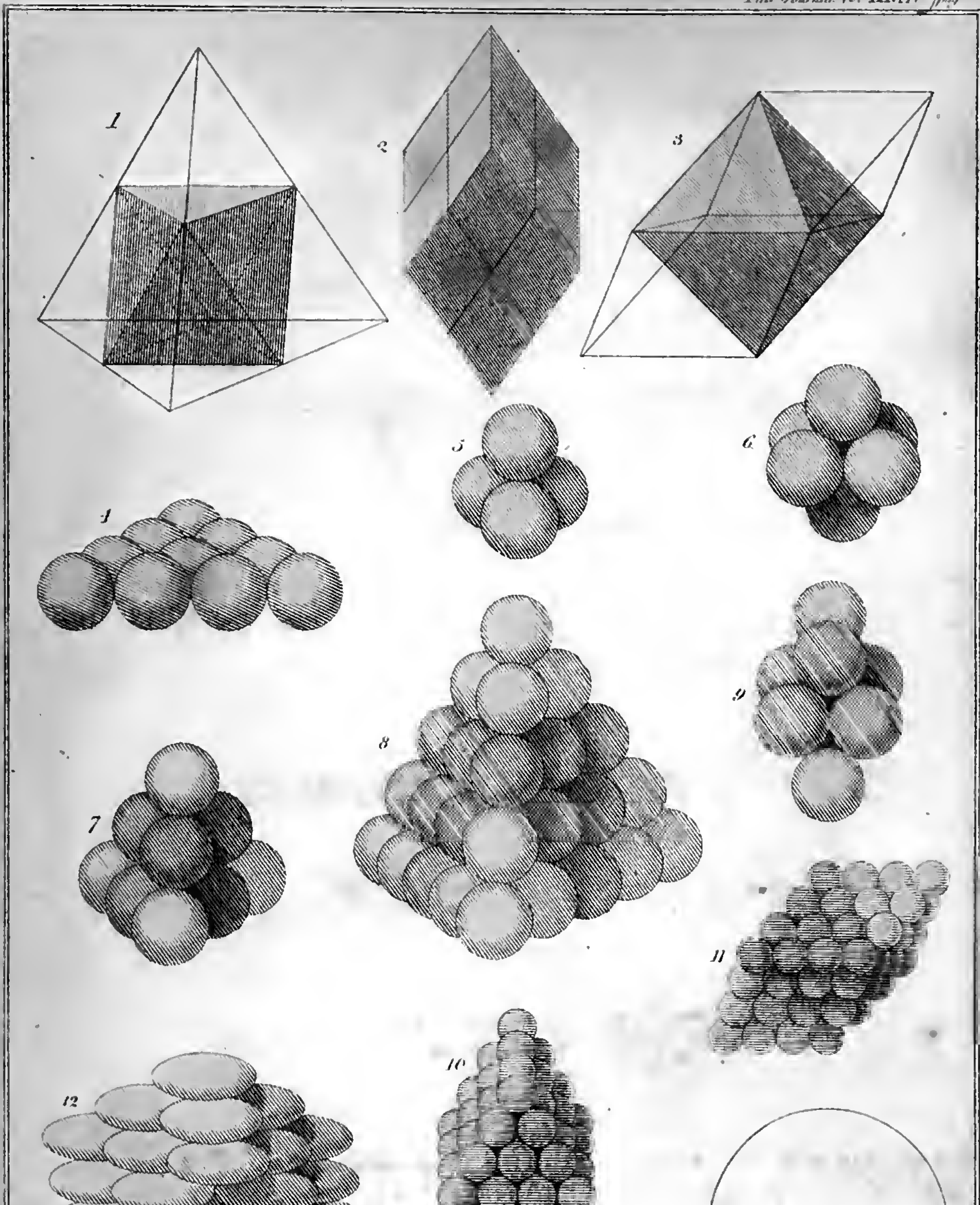
### SEPTEMBER, 1813.



OCTOBER

DECEMBER, 1813.

## SUPPLEMENT TO VOL. XXXVI.


1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
F
I
C
T

A

# JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

*SEPTEMBER*, 1813.

## ARTICLE I.

### *On common Ink for writing. By Dr.* Bancroft, *F. R. S. &c. From his Researches into Permanent Colours.*

*(Concluded from p. 237 of Vol. XXXV.)*

M. CHAPTAL would reject the use of sugar in making ink, as being completely useless; for to him it did not seem even to render the ink more glossy or shining, though such an effect has always been manifest to my apprehension. He found no benefit by employing either vinegar, wine, or beer, instead of water, as a vehicle of the colouring matter of ink; indeed, he was persuaded that beer did harm, by increasing the disposition of ink made with it, to become mouldy.

Best proportions and ingredients for ink by Chaptal.

M. Chaptal concludes, from his numerous experiments in regard to writing ink*, that the best ingredients and proportions for composing it, are the following, viz. Two parts of galls in sorts, bruised, and one part of logwood chipped; these are to be

* See his Chimie appliquée aux Arts, tom. iv.

Chaptal's ink. boiled in twenty-five times their weight of water, for the space of two hours, adding a little water, from time to time, according to the evaporation. The decoction so made, he says, will commonly mark from 3 to $3\frac{1}{2}$ degrees upon the hydrometer of Beaumé, equal to about 1022 of the common standard. At the same time, a solution of gum arabic is to be made with warm water, until the latter will dissolve no more of the former. This solution will mark 14 or 15 degrees, equal to about 1110. A solution of calcined sulphate of iron is also to be made and concentrated so that it will mark 10 degrees, equal to about 1071. And to this, as much sulphate of copper is to be added, as will be equal to one-fifteenth part of the galls employed to make the decoction. The several matters being so prepared, six measures of the decoction are to be mixed with four measures of the solution of gum, and to this mixture, from three to four measures of the metallic solution are to be added, by a little at a time, mixing the several matters each time by shaking. Ink so made will, he says, form no sediment; and he estimates the proportions of solid matters contained in it to be 500 parts of gum, 462 parts of the extract of galls and logwood, and 481 parts of metallic oxides.

But though the hydrometer may enable those who will employ it, to obviate or correct the uncertainties resulting from the difference of quality in galls, some persons may wish to avoid the trouble of doing so, and choose rather to incur the risk of some little defect in the ink they may prepare; and for their satisfaction, therefore, I will mention the following, as being generally the most suitable proportions for composing the best and most lasting writing ink, viz.

Proportions of ink by the author. Sulphate of iron, galls, and sugar.

Take of good Aleppo gall, in sorts, coarsely powdered, twelve ounces, and of chipped logwood six ounces; boil these in five quarts of soft water, two hours, and strain off the decoction whilst hot; then put to the residuum as much boiling water as, when properly stirred, strained, and added to the former, will suffice to make the whole of the decoction equal to one gallon; add to this five ounces of sulphate of iron, with the same quantity of gum arabic, and two ounces of good dry musco-

muscovada sugar; let these be all dissolved and well mixed by stirring.

I do not consider a calcination of the sulphate of iron, which Chaptal, Proust, and some others, have recommended, as of much importance; for though the ink may be thereby made to attain its *utmost* degree of blackness, almost immediately, the strong disposition which ink has to absorb oxigen from the atmosphere, until saturated therewith, will enable it, without such calcination, to attain an equal degree of blackness in a day or two, according to the temperature of the air, if the latter be allowed free access to it. I have omitted the sulphate of copper, for the reasons lately mentioned; and if any portion of that metal were deemed beneficial, I should prefer verdigrise to the sulphate, the latter containing a much larger proportion of acid, than even the sulphate of iron, and being, therefore, more likely to render the ink corrosive.

The sulphate need not be calcined.

Some persons have recommended the addition of indigo to ink; but unless it be previously dissolved by sulphuric acid, it will be found to subside, even though very finely powdered; and, if so dissolved, this increased portion of acid will render the ink much more corrosive; and after all, the blue afforded by this combination (as was formerly noticed in regard to the sulphate of indigo) will not prove very durable.

Indigo is not advisable.

Gum is highly useful to retard the separation and subsidence of its black part, or compound of colouring matter and iron, previous to its application to paper, as well as to hinder it from spreading and penetrating too far into the latter, when applied to it.

As the acid part of galls is extracted more readily than the other soluble parts, especially when the water employed for that purpose is cold, and as ink, which, along with colouring matter, contains more than the ordinary proportion of this acid, is the least disposed to produce a sediment (for the reasons lately assigned) some persons have recommended the making of it by *a cold* infusion of galls. But when this is done, the galls must be employed in a much greater proportion; and even with this additional expence, there will be cause to fear, that

Cold infusion rejected.

Considerations as to the ingredients and their proportions.

ink, so obtained, may not prove so durable as other ink containing a full proportion of the less soluble, but more stable, part of the colouring matter of galls. It is, therefore, with good reason, that Lewis, Chaptal, and others, have recommended *boiling* to extract the *whole* of the soluble matter contained in galls, especially as oxigen will be absorbed during the ebullition, and this absorption will contribute to give the ink its full degree of blackness so much the more speedily.

Ink, in which the colouring matter of logwood bears a large proportion, will be made red by applying muriatic acid to it; this redness, however, will soon disappear, and the former blackness be restored, partly, at least, in consequence of the volatility of the acid.

It does not appear, that any considerable advantage is gained by substituting any of the other acids for the sulphuric, to dissolve iron for making ink, though the case is different in regard to dyeing and calico-printing.

I have already observed, that an excess of sulphate of iron produces ink of a *blueish* tint, which, if the excess be great, will, at no remote period, become yellow; probably, because the affinity of the metallic oxide for oxigen, not being counteracted by a sufficient portion of vegetable matter, the latter will gradually suffer a decomposition from the excess of oxigen absorbed, and at length the oxidated iron alone will remain. A similar effect will, indeed, take place, after a long course of years, even when there is no disproportion of iron; but it will be retarded by increasing the proportion of galls beyond that which produces the blackest colour: and, indeed, by such an increase as to make the ink incline considerably from what is deemed a good black, towards a brownish purple.

But, unfortunately, that ink in which the proportion of galls is greatest, is the most disposed to become mouldy; a defect which it is difficult, if not impossible, to hinder, in any considerable degree, so long as ink retains the mucilaginous part of the galls, which water always extracts along with the colouring matter. It has, indeed, been found, that by keeping a saturated infusion or decoction of galls, six or eight months, more or less, according

according to the temperature of the atmosphere, and carefully removing the mouldy pellicle, when it forms on its surface, from time to time, the mucilage will at length be wholly separated from the remaining part of the infusion or decoction, and no subsequent production of mouldiness will then take place therein; and if it be afterwards passed through a fine strainer, and mixed with a suitable proportion of sulphate of iron and of gum arabic, an ink may be formed, which will be exempted from the defect of mouldiness. It was, probably, in this way that the ink sold in France under the name of *encre de Guyot*, was prepared; and Dr. Tarry has, in the Ann. de Chimie, tom. 74, given a receipe for preparing an ink on this principle. It must, however, be observed, that by this method of separating the mucilage from the colouring matter of the galls, a considerable portion of the latter will be also taken away and lost; and there is room to suspect, that the remainder, by its having been kept so long in a fluid state, will have suffered some degree of deterioration, tending to render the colour of the ink, when made with it, less durable. And, therefore, knowing that the mucilaginous part of the galls does not combine with the iron and colouring matter in the formation of ink, I thought it might be practicable and advantageous to separate the former from the latter, by adding just enough of *caustic* potash to the ink to neutralize the sulphuric acid, and throw down the black compound of iron and vegetable colouring matter; and after separating this last by a filtre, or fine strainer, redissolving, converting it again to ink with sulphuric acid, sufficiently diluted, or with *distilled* vinegar, avoiding that which had not been *distilled*, as it would restore other mucilage almost as hurtful as that which this process might separate. And, having produced an ink in this way, I found it quite unobjectionable, and free from all disposition to become mouldy.

Ink in which the mucilage has been decomposed;

probably less durable.

Newman formerly recommended the adding of cloves to ink, in order to counteract its disposition to mouldiness; and the late Dr. Black adopted this recommendation, advising only, that the cloves should be powdered, and rubbed in a mortar with the mucilage of gum arabic, to render their essential oil miscible

Oil of cloves against mouldiness.

miscible with the ink; and by this expedient he supposed that an ink might be obtained, which would be but little, if at all, subject to the defect in question. I did not, however, in repeating this experiment, find any considerable benefit from cloves, employed in this way, and therefore substituted camphire, which seemed to answer better, though it appeared to give the ink a blueish tint; but I have been since convinced, that neither these, nor any other means, will completely obviate the mouldiness in question, so long as ink retains the *mucilaginous* part of galls *in a liquid state.*

Indestructible ink of Chaptal. Lamp black, glue, and a little salt.

M. Chaptal observes*, that since the oxygenated muriatic acid has been found capable of discharging the colour of common writing ink both from parchment and paper, without injuring their texture, it has been fraudulently employed to efface particular parts or words of deeds, contracts, and other writings, for which others have been substituted, leaving the signatures untouched: and that, in consequence of these frauds, the commercial part of society, as well as governments, became solicitous for the discovery of some composition, which might be employed instead of common writing ink, without its defects, and therefore, (being then minister of the interior of France, and possessed of great chemical science) he, as might be expected, occupied himself particularly with that subject; and he states, that up to the then present time, the composition which had been found most useful for this purpose, consisted of a solution of glue in water, with which a sufficient portion of lamp-black, and a little sea-salt were intimately mixed by rubbing them together on marble. This composition was made sufficiently thin (by water) to flow readily from the pen; and he describes it as being "*d'un trés bon usage*;" and capable of resisting the action, not merely of cold, but of boiling water; and also of acids, alkalies, and spirit of wine; and attended with no inconvenience, but that of abrasion, by being rubbed. ("Elle n'a que l'inconvenient de s'estomper par le frottement.")

Though I have never made trial of this composition, I can

* Chemie appliquée aux Arts, tom. iv. p. 244.

readily

readily believe M. Chaptal's account of its good properties; but I must observe, that it differs from the ink commonly used when Pliny wrote, in nothing but the addition of sea-salt, (for which, as being less disposed to deliquate, I should think either saltpetre or sulphate of potash, might be advantageously substituted) and in the employment of *glue* instead of gum arabic (which Pliny recommends) to give the composition sufficient tenacity and consistency. Indeed, Pliny, as I lately mentioned, directs glue to be employed with lamp-black instead of gum, when the atramentum, or black mixture was intended to be applied as a pigment internally to the walls, &c. of houses. I have, indeed, found, that when lamp-black has been incorporated with common ink, by first rubbing the former in a mortar, with a mucilage of gum arabic, the writing done with it could not be rendered invisible by the application of muriatic acid; and, doubtless, such an addition of lamp-black would hinder the letters from ever becoming illegible by age, at least within any length of time which the paper and parchment could be expected to last. But ink made with this addition would require to be frequently shaken or stirred, as the lamp-black would otherwise be apt to separate and subside. Glue could not be advantageously employed with any ink containing tannin, for obvious reasons.

This differs little from the ink of Pliny.

As all inks in which the colouring matter is mixed with an aqueous menstruum or vehicle, are liable to suffer injury by wetting, I resolved to make trial of the essential oil, or spirit of turpentine, and to incorporate with it, as intimately as possible, a sufficient portion of finely-powdered lamp-black; and having done so, I obtained an ink which proved to be sufficiently black, and flowed from the pen readily, and with a remarkably smooth and homogeneal effect. I have, indeed, now before me several pieces of writing, for which this composition was employed (dated at Kew, in September, 1799) and the strokes of the pen, though fine, are as distinct and even as possible. Strong nitric and muriatic acids have been applied to different parts of the writing, without impairing the colour in the slightest degree; nor did boiling water cause the letters to

Essential oil, or spirit of turpentine, and lamp black, make a very durable ink.

run

run or spread. The most concentrated sulphuric acid, or oil of vitriol, being dropped upon the writing, and suffered to remain several days, was found to have nearly destroyed the paper, but not the writing. And I cannot conceive any purpose, depending upon the fixity, durability, and indestructibility of ink, which may not be answered by this composition, there being, as I am persuaded, no chemical agent, nor any length of time, which can efface its impressions, without destroying the paper or parchment on which they are made.

East Indian and other inks.

In some parts of the East Indies, a permanent writing ink is formed, by dissolving the brownish black liquid contained in the oriental marking nuts, (semecarpus anacardium,) mentioned at p. 308 of my first volume. The solution is to be made by an alkaline lixivium, and afterwards neutralized by vinegar.

There are some few instances of inks said to be produced by vegetable colouring matters upon the basis of alumine instead of iron : one of these, first mentioned by Ray, and afterwards by Linnæus, is from the poisonous berries of the actea spicata, or common black-berried herb Christopher. Linnæus also mentions the fruit of the impetrum procumbens, as affording another such ink. I believe, however, that neither of these can be lasting.

Barham, in his Hortus Americanus, mentions the pods of the mimosa juliflora, improperly called poponax in Jamaica) as affording a good ink on a similar basis. He says, "they soak some of the pods all night in water, then mix a little alum with it, and boil it to a due thickness, which makes a very fine black and strong ink ;" and he adds, that he had often made and written with it. Reflecting, however, upon the family and genus to which this tree belongs, I am persuaded, that the black which it affords must be produced by iron, which might very easily be dissolved, partly by the astringent vegetable matter, and partly by the acid part of the alum contained in the infusion, while the latter underwent the *boiling* which is prescribed, if performed, as it doubtless must have been, in an iron vessel ; and this, probably, is the fact, also, in regard to the natural ink, which the inspissated juice of the old trees of the fagus castanea,

Lin.

Lin. is said to afford. The inspissation or evaporation of this being, doubtless, performed also in iron vessels; though Crell has supposed, that the juice of this tree naturally contains iron; but certainly it cannot contain it in any proportion sufficient to produce such an effect.

---

II.

*Inquiries concerning the mutual Decomposition of soluble and insoluble Salts. By M.* DULONG.

(*Continued from p.* 280.)

Mutual decomposition of the insoluble neutral salts, &c.

THE carbonates of strontian and of lead present an apparent anomaly with the sulphates of potash and soda. For if a great excess be used of either of these soluble salts, but a very small quantity of the insoluble sulphate is formed. The liquid, when filtered, after the operation, is only, in a slight degree, alkaline, and does not contain any carbonic acid. The same thing occurs with strontian, as the sulphate of that base is evidently more soluble than the carbonate, so that when the carbonic acid can no longer pass through the liquid without being absorbed, all ulterior decomposition becomes impossible, for the sulphate of strontian which would be formed, will be immediately decomposed by the carbonate of potash contained in the fluid.

The carbonate of lead comports itself in the same manner as the preceding, though the insolubility of the sulphate and carbonate of lead, considered with regard to water, are sensibly the same. M. Berthollet has already remarked, that the sulphate of lead is more soluble than the muriate of the same metal in an alkaline solution*, and I have perceived, that there is a still more considerable difference between the solubility of the sulphate and carbonate under the same circumstances.

* Mem. de l'Institut, tom. III, p. 221.

When,

Mutual decomposition of the insoluble neutral salts, &c.

When, therefore, the first portions of the sulphuric acid have been precipitated from the sulphate of soda or potash, the liquor, becoming by that means charged with excess of alkali, dissolves a part of the sulphate of lead; and if it contained carbonic acid, this sulphate would be immediately transformed into a carbonate that would be precipitated. At this period, therefore, the decomposition can make no further progress.

For the same reason, the results of the inverse experiments differ from those which are presented by all the other salts. There is no limit in the action of the sub-carbonate of potash, or of soda, on the sulphate of lead; but the decomposition continues till there is evidently no longer any carbonic acid in the liquid.

The explanation of the two preceding anomalies is still further confirmed by the decomposition of these two carbonates by the sulphates of magnesia and ammonia, of which the bases form, with carbonic acid, an insoluble or volatile salt, which, by separating itself from the liquid, in proportion as it is formed, continues to restore the primitive circumstances.

I have analysed several salts that have been thus regenerated, from their base of barytes, strontian, and of lime, and their proportions did not appear to me to differ from those which they had when they were obtained by direct precipitation; but it is not so with some of the metallic salts.

If a perfectly neutral solution of the chromate of potash be poured upon an excess of carbonate of lead well pulverised, these two salts are mutually decomposed even at the ordinary temperature of from 10° to 15°. In this case, the carbonic acid is not disengaged, the liquid turns a pale yellow, and becomes very alkaline, and the precipitate at the same time takes a yellow colour, and contains chromate of lead. The equilibrium is, without delay, established, and the carbonate of lead does not experience any further alteration, although there is still some chromate of potash in solution.

But if the experiment be performed at the temperature of ebullition, and on the contrary an excess of the chromate of potash be employed, the chromate of lead which is formed almost

almost instantaneously, is soon transformed into a crimson powder, which is composed of crystals, which are too small to be distinguished as to their figure, even with a magnifier. The liquid becomes alkaline as in the preceding experiment, and likewise contains the sub-carbonate of potash; the specific gravity of this red precipitate is much greater than that of the chromate of lead, it is completely insoluble, and does not effervesce with the nitric acid, but it immediately turns to a yellow colour, and yields the oxide of lead to this acid. I shall confine myself in the present instance to observe, that it is a sub-chromate similar in its proportions to the sub-carbonates, that is to say, in which the chromic acid is combined with a quantity of the oxide of lead, exactly double that which is found in the neutral chromate.

Mutual decomposition of the insoluble neutral salts, &c.

I shall return in a particular memoir to this salt, and several others of which I have had an opportunity of verifying the existence: these details at present would lead me too far from my subject.

The result of all the preceding facts is—1st. that all the insoluble salts are decomposed by the subcarbonates of potash and of soda, but that a mutual exchange of the principles of these salts cannot, in any case, be completely made; or, in other words, that the decomposition of the sub-carbonates is only partial; 2d. that all the soluble salts, of which the acid forms, with the base of the insoluble carbonates, an insoluble salt, are decomposed by these carbonates, until the decomposition has reached a certain limit which it cannot pass.

Result of the preceding facts.

We shall now endeavour to trace the origin of these phenomena in appearancee so contradictory.

On a first examination it is evident these decompositions differ essentially from all those that have been hitherto observed. If it has already been remarked, that the reciprocal action of two salts may give rise to opposite results, the cause has been found to consist in a difference of the circumstances necessary to obtain such or such a result. The different degrees of tem-

perature

Mutual decomposition of the insoluble neutral salts, &c.

perature which do not affect equally the elements of the salts, and the variety in the nature of the solvents, are sufficient to explain all those cases formerly designated by the name of anomalies. But in that at present in question, these two circumstances are the same. It is at the temperature of the ebullition of water, and in the midst of this liquid, that the opposite phenomena are produced.

I shall further observe, that neither of these decompositions can be attributed to the simultaneous concourse of affinities of the elements which are united when they take place. The author of the chemical statics, (Berthollet) with the assistance of some very ingenious experiments and considerations, has shewn, that the difference of solubility of the compositions which may result from the mixture of two soluble salts, is always sufficient to explain their decomposition, and that it is useless to refer to the consideration of the different degrees of affinity of their elements, and besides that they cannot be exactly appreciated by any known method.

The experiments related at the commencement of this memoir prove, in a very evident manner, that the decomposition of the insoluble salts by the soluble carbonates can no longer depend upon a similar concurrence. In fact, it is now well known, that the soluble sub-carbonates have such proportions, that their principles may be exchanged exactly with those of all the neutral insoluble salts; so that if the decomposition were complete, the result would be on one side, an insoluble sub-carbonate analogous in its proportions to the soluble sub-carbonates, and on the other, a neutral salt of potash or soda. Now, if the decomposition depended upon the simultaneous concurrence of the divellent affinities, these powers being supposed constant between the same principles, there would be no reason why the decomposition should not proceed to the very end in the same manner; and as no insoluble salt can totally decompose a soluble sub-carbonate, it becomes a consequence that this decomposition should depend upon some other cause. From the inverse experiment, that is to say, the decomposition

of

of insoluble carbonates by insoluble salts, an equally strong argument may be deduced against this theory.

Mutual decomposition of the insoluble neutral salts, &c.

Since the reaction of these bodies ceases at a certain period of the operation, we may conclude, that the powers which decide it, undergo some modification dependent upon the progress even of the decomposition.

Now, during the course of this decomposition, only one remarkable change takes place, that of the state of saturation of the liquid.

When a soluble sub-carbonate acts on an insoluble salt, in proportion as the carbonic acid is precipitated on the base of the insoluble salt, it is replaced in the solution by a quantity of another acid, capable of exactly neutralizing the alkali with which it constitutes a sub-carbonate. Thus, during the whole course of the decomposition, fresh quantities of neutral salt replace the corresponding quantities of an alkaline salt, and if we consider the alkali, which exceeds the neutralization of the carbonic acid, in the sub-carbonate which is not decomposed, as acting upon the two acids, it is evident, that in proportion as the decomposition advances, the liquid approaches more and more to the neutral or saturated state. In the inverse experiment, a contrary alteration is seen; each portion of the acid of the soluble salt, which is precipitated on the base of the insoluble carbonate, is replaced by a quantity of carbonic acid, which forms, with the corresponding base, a perfect sub-carbonate; and the more the acid is precipitated upon the insoluble carbonate, the more sub-carbonate the liquid contains, and the more its state of saturation is distant from neutralization.

This consideration seems to lead directly to the following explanation.

It is known, that all the salts, even those which possess the greatest cohesion, yield to potash or caustic soda, a more or less considerable portion of their acid, according to circumstances. Now, the soluble sub-carbonates may be considered as weak alkalies, which may take from all the insoluble salts a small quantity of their acids; this effect would soon be limited,

if

Mutual decomposition of the insoluble neutral salts, &c.

if the alkali was pure, by the resistance progressively arising from the base. But the latter meeting in the liquid an acid with which it can form an insoluble sub-salt, unites with it, and thus re-establishes the primitive conditions of the experiment; the same effect is produced successively on the new portions of substances until the degree of saturation of the liquid is in equilibrium with the power of cohesion of the insoluble salt, so that the less this resistance is, the more progress the decomposition will make.

It may not, perhaps, be evident why the base of the insoluble salt having abandoned its acid to the dissolved alkali, should, at the same time, take from it another acid. But I shall observe, that the insoluble carbonate which is the result of this action, being naturally with an excess of base, and at the same degree of saturation as the soluble carbonate, the latter cannot oppose any resistance to the formation of the first. It would not be the same if the liquid contained an acid which would not form a neutral salt with the base of the insoluble salt. Experience has demonstrated to me, that the soluble salts, which cannot decompose when they are neutral, a given insoluble salt, in the same manner, cannot when they are with an excess of alkali. The following experiment seems to confirm this reasoning still further.

*Experiment F.*—A solution of caustic potash, which retained sufficient carbonic acid to produce a remarkable effervescence with the acids, was boiled for the space of an hour upon the sulphate of barytes carefully pulverized. The fluid separated from the precipitate contained sulphuric acid; but concentrated nitric acid being poured upon the insoluble matter, it did not disengage the smallest bubble of gas, though this acid had dissolved the barytes. It is evident, that in this case the alkali being too distant from the degree of saturation which belongs to the sub-carbonates, opposes even the formation of this alkaline salt, and all the action of this kind of subcarbonate is reduced to taking up a certain quantity of sulphuric acid from the sulphate of barytes.

The inverse experiment is explained with the same facility. The

The insoluble carbonates have, as is known, a composition analogous to that of the soluble sub-carbonates; and though the force of the cohesion they possess, diminishes the energy of the alkali they contain in excess, it does not entirely destroy it. When an insoluble carbonate is in contact with a neutral soluble salt, the base of the carbonate must tend to take part of the acid of the neutral salt; and if, from this union, an insoluble salt can result, the force of cohesion peculiar to this compound, will determine the formation. The carbonic acid, of which the elasticity is no longer overcome by the affinity of the base, combined with a more fixed acid, escapes in the form of gas— the same effect being produced on fresh quantities, the liquid becomes sufficiently alkaline to absorb the carbonic acid in its nascent state; and then the sub-carbonate of potash or of soda is formed, which replaces the decomposed neutral salt. The precipitation of the acid on the insoluble carbonate, and the absorption of the carbonic acid by the liquid, continues until the resistance which the excess of alkali so developed opposes the precipitation of the acid, and forms an equilibrium to the force by which that precipitation was effected. All action then ceases, so that the more cohesion the insoluble salt possesses, the greater will be the proportion of acid taken from the soluble salt.

Mutual decomposition of the insoluble neutral salts, &c.

By admitting this explanation, all the phenomena which belong to these decompositions, are perfectly accounted for. We see, first, why all the insoluble salts, without exception, are decomposed by the sub-carbonates of potash and soda, because there is not one of these salts which cannot be decomposed partially by potash or soda; secondly, that the sub-carbonates which I have named are the only ones that can possess this general property, because the carbonic is the only acid which can form insoluble compounds with the bases of all the insoluble salts without neutralizing them. Thirdly, why the addition of potash or caustic soda makes the decomposition make fresh progress; for, in this case, by the increase of the mass of alkali, a fresh quantity of insoluble carbonate should be formed. But from the experiment (F) it is seen, that by the

suc-

Mutual decomposition of the insoluble neutral salts, &c.

successive additions of alkali, the liquid can never be deprived of the whole of the carbonic acid it contains.

If these results be compared with experiments C and E, it will be seen, that the solution which can no longer act upon the insoluble salt, (exp. C) is more alkaline than that which has no longer any action upon the insoluble carbonate (exp. E.) This difference may be measured by the proportion of the quantities of carbonic acid and sulphuric acid, which are then found in the two fluids. When the carbonate of potash can no longer decompose the sulphate of barytes, the carbonic acid, which remains in the solution, is to the sulphuric acid nearly as : : 3 : 1, and when the sulphate of potash can no longer act upon the carbonate of barytes, these two acids are nearly in the same relation as 3 to 2; from whence it follows, that the first liquor is much more alkaline than the second.

It is easy to account for this difference, by examining the conditions of the equilibrium established in the two cases. When the sulphate of potash no longer decomposes the carbonate of barytes, it is because the excess of alkali, which is developed in the liquid, can form an equilibrium against the power with which the sulphate of barytes tends to form itself under these circumstances. When the sub-carbonate of potash can no longer decompose the sulphate of barytes, it is in consequence of the excess of alkali in the liquid not being sufficiently considerable to overcome the cohesion peculiar to this salt. Now, we know that, in order to overcome the effect of the cohesion of a body, when this effect is accomplished, it requires a greater force than that which will be precisely necessary to oppose the accomplishment of the same effect. Therefore, the sub-carbonate of potash ought to cease to decompose the sulphate of barytes, before the sulphuric and carbonic acids are in that relation in which these two acids are found when the equilibrium is established by the inverse experiment.

Hence we may deduce this inference, that a mixture of sulphate of potash, and of sub-carbonate of potash, in which the proportions of the sulphuric acid and carbonic acids shall be between the two limits which I have pointed out, will have no

action

action either on the sulphate of barytes, or on the carbonate of the same base. This is what experiment likewise confirms.

Mutual decompositions of the insoluble neutral salts, &c.

I have already observed, that for the other insoluble salts there will be other relations of quantity; but there is always a certain interval, more or less considerable, between their limits.

The sub-carbonates of potash and soda being likewise able to decompose all the insoluble salts in the dry way, and this decomposition depending on properties different from those upon which the decomposition is effected in the humid way, it was interesting to compare the results of these two different modes of decomposition.

*Experiment G.*—I heated to ignition for an hour in a crucible of platina, a mixture of the sub-carbonate of soda, and sulphate of barytes in excess. The two salts entered into a state of fusion; and after cooling, the mass was pulverized and placed in a filter, on which boiling water was poured. I expected to find the liquor neutralized, or not far distant from that state; but, on the contrary, it was strongly alkaline, and effervesced briskly with the acids. On analysing it, I found that it contained carbonic and sulphuric acid in the same proportions as in a solution of the sulphate of soda exhausted upon the carbonate of barytes. This evidently arises from the sulphate of soda, in proportion as it dissolves, re-acting on the carbonate of barytes, with which it finds itself in contact, and this reaction is almost instantaneous. It is sufficient to pour a boiling solution of neutral sulphate of soda on the carbonate of barytes placed in a filtre, in order that more than three quarters of the sulphuric acid may be precipitated and replaced by a corresponding quantity of carbonic acid. It is, therefore, impossible to verify by experiment whether, as the theory indicates, the exchange of the base and the acid be completely made between the soluble sub-carbonates and insoluble salts, in operating by the dry way; but it is seen, that the decomposition proceeds much farther than in the humid way.

I shall terminate this memoir with some observations on the measure of affinities, and on the applications that may be deduced from the theory I have been explaining.

Since the quantity of sub-carbonate of potash and of soda

Mutual decompositions of the insoluble neutral salts, &c.

employed in experiment C, contained equal quantities of carbonic acid, the ponderable masses of potash and of soda, which enter into the composition of these salts, were therefore in inverse ratio to their capacity of saturation ; and, as these two alkalies were likewise in the same state of saturation, their effect ought to be the same if the affinity was exactly proportioned to the capacity for saturation ; for, whatever may be, in this case, the circumstances which modify the affinity; as they are perfectly identical as to both, the results ought to preserve their equality. Nevertheless, the quantities of the sulphate of barytes, which is decomposed, or, what is the same thing, the quantities of sulphuric acid taken up by the potash or the soda, in this experiment, are nearly in the relation of 6 to 5. The experiment (E) also confirms this result. The quantities of potash and of soda are likewise found to be in the inverse ratio of the capacities for saturation. The circumstances were the same on both sides, and the ponderable masses of acid retained by those two alkalies, are likewise in the same proportion. If the analysis upon which the proportions depend, were perfectly accurate, we should be forced to admit, that the affinities of these two alkalies is not exactly proportioned to their capacity for saturation.

The re-action of the soluble salts of potash and of soda upon the insoluble carbonates, considered as a general property that is applicable to all the salts which unite the condition already mentioned, will furnish, in several instances, much shorter, and more exact means of analysis, than those which result from the facts already known : but there may be deduced from the theory which I have explained, a more important application, which I shall only point out at present, because it ought to form the subject of a second memoir.

After having ascertained, by direct experiments, the reciprocal decomposition of a great number of soluble and insoluble salts, I have sought for the explanation of this phenomenon, and the means of foreseeing the results. The analogy founded upon the corresponding phenomena which took place between the soluble salts, naturally led me to consider the various degrees

grees of cohesion belonging to each insoluble salt as the cause of these decompositions. It may, in fact, be conceived, that the cohesion of two salts equally insoluble, may be very different; and that if an insoluble salt comes in contact with a soluble salt, of which the principles, by being reciprocally exchanged with those of the former, can produce another insoluble salt, possessed of a much greater cohesion, there will be a decomposition.

Mutual decomposition of the insoluble neutral salts, &c.

If, then, it were possible, by any method, to appreciate the different degrees of cohesion belonging to each insoluble salt, in the same manner as the different degrees of solubility of those which are soluble are estimated, the decomposition of those salts which contain the above-mentioned conditions, would be foreseen with as much facility as that of the soluble salts. Now the results of the decomposition of the insoluble carbonates by the insoluble salts, presents a simple method, if not of estimating the absolute intensity of this power, at least of ascertaining the differences which are seen, in this respect, in the insoluble salts. When a soluble salt ceases to decompose an insoluble carbonate, there is an equilibrium between the power with which the insoluble salt tends to precipitate itself, and the excess of alkali developed in the solution; and the result, as we have already said, is, that the greater this tendency to precipitation, the more the excess of alkali which is developed will be considerable. If, therefore, for each insoluble salt, the relation were determined which exists between the quantity which is regenerated, and the total quantity of salt that might have been formed by the entire precipitation of the acid, then by comparing the various relations obtained for all those salts formed with the same base, a scale of their cohesion might be easily arranged, and according to the rank which a given salt would hold in this scale, it might be known what were the soluble salts that would decompose it.

I have already ascertained a considerably large number of these relations, and their indications have been fully confirmed by experiments. But I shall delay presenting this work to the Class until it has included, if not all the known salts, at least the greatest part of those which are frequently met with in analysis.

 METE-

# METEOROLOGICAL JOURNAL.

| 1813. | Wind. | Barometer. Max. | Min. | Med. | Thermometer. Max. | Min. | Med. | Evap. | Rain. | |
|---|---|---|---|---|---|---|---|---|---|---|
| 6th Mo. | | | | | | | | | | |
| June 21 | N E | 30·16 | 30·14 | 30·150 | 66 | 46 | 56·0 | —— | | ☾ |
| 22 | N E | 30·18 | 30·14 | 30·160 | 67 | 41 | 54·0 | —— | | |
| 23 | N E | 30·10 | 30·08 | 30·090 | 70 | 55 | 62·5 | —— | | |
| 24 | N E | 30·15 | 30·08 | 30·115 | 64 | 46 | 55·0 | —— | —— | |
| 25 | E | 30·18 | 30·15 | 30·165 | 74 | 50 | 62·0 | ·55 | 4 | |
| 26 | N E | 30·18 | 30·10 | 30·140 | 74 | 48 | 61·0 | —— | | |
| 27 | E | 30·10 | 29·97 | 30·035 | 77 | 45 | 61·0 | —— | | |
| 28 | Var. | 29·97 | 29·77 | 29·870 | 75 | 53 | 64·0 | —— | ·37 | ● |
| 29 | S W | 29·77 | 29·64 | 29·705 | 74 | 52 | 63·0 | —— | ·80 | |
| 30 | W | 29·64 | 29·54 | 29·590 | 65 | 52 | 58·5 | ·40 | ·75 | |
| 7th Mo. | | | | | | | | | | |
| July 1 | N W | 29·75 | 29·64 | 29·695 | 70 | 53 | 61·5 | —— | ·16 | |
| 2 | N | 29·86 | 29·75 | 29·805 | 67 | 50 | 58·5 | —— | ·20 | |
| 3 | N W | 30·11 | 29·86 | 29·985 | 73 | 42 | 57·5 | ·25 | | |
| 4 | N W | 30·18 | 30·11 | 30·145 | 70 | 50 | 60·0 | —— | ·2 | |
| 5 | W | 30·18 | 30·04 | 30·110 | 74 | 47 | 60·5 | —— | | |
| 6 | S W | 30·04 | 29·74 | 29·890 | 78 | 56 | 67·0 | ·35 | | ☽ |
| 7 | S | 29·65 | 29·57 | 29·610 | 78 | 57 | 67·5 | —— | | |
| 8 | W | 29·59 | 29·55 | 29·570 | 75 | 55 | 65·0 | —— | —— | |
| 9 | W | 29·83 | 29·59 | 29·710 | 79 | 54 | 66·5 | ·36 | | |
| 10 | N W | 29·91 | 29·83 | 29·870 | 78 | 51 | 64·5 | —— | | |
| 11 | N W | 29·93 | 29·90 | 29·915 | 77 | 51 | 64·0 | —— | | |
| 12 | W | 29·90 | 29·76 | 29·830 | 76 | 51 | 63·5 | —— | | |
| 13 | S W | 29·76 | 29·63 | 29·695 | 76 | 58 | 67·0 | ·38 | | ○ |
| 14 | S W | 29·63 | 29·60 | 29·615 | 71 | 58 | 64·5 | —— | | |
| 15 | N W | 29·70 | 29·60 | 29·650 | 67 | 50 | 58·5 | —— | ·60 | |
| 16 | N W | 29·82 | 29·70 | 29·760 | 73 | 50 | 61·5 | —— | ·10 | |
| 17 | N W | 29·87 | 29·82 | 29·845 | 74 | 47 | 60·5 | —— | | |
| 18 | W | 29·87 | 29·73 | 29·800 | 73 | 50 | 61·5 | .43 | | |
| 19 | W | 29·73 | —— | —— | 73 | —— | —— | —— | —— | |
| | | 30·18 | 29·54 | 29·875 | 79 | 41 | 61·69 | 2·72 | 3·04 | |

## REMARKS.

*Sixth Month*, 21. Brisk wind through the day. 22. Wind more gentle Cumulostratus and Cirrostratus. 24. A shower about 1 p. m. 25. The wind inclines to S E. Clear twilight, somewhat orange coloured. 27. Cirrus, cnanging to Cirrocumulus and Cirrostratus: twilight, somewhat opake, but coloured. 28. Wind N. E. a. m. The Stratus colour appears to have prevailed in the night. Slight showers at intervals during the day. At 7 p. m. several Nimbi, and some thunder to the S. W. which, with occasional lightning passed by S. to N. E. At 9 p. m. the air was so loaded with vapour as to deposit water on a glass vessel cooled only to 58·5°. It now began to rain heavily; ceasing at 10, with thunder and lightning still in the N. 29. Cirrus, Cirrostratus and Cumulostratus. About one p. m. a heavy storm of rain and hail, with several electrical discharges. 30. In the forenoon heavy rain, ushered in by a peculiar hollow sound in the Wind, then southerly: wet at intervals, p. m. A part of this day's rain was taken by estimation, the guage having been left under cover.

*Seventh Month*, 3. After an appearance of two distinct orders of cloud during the forenoon, inosculation took place suddenly, about one, and the Cumulostratus, with a brisk wind, prevailed till sun set. 4. A slight shower p. m. From the 5th to the 9th several kinds of cloud prevailed and occasioned at times considerable indications of rain; of which, however, a few drops only fell, the clouds still passing away to the N. In that quarter, on the evening of 9th we had several distant Nimbi, with the usual appearances of a strong electric charge. A single flash of lightning and some rain, just discernible in the horizon, were the only results. 13. After repeated exhibitions of the Cumulostratus, which continued to pass over to the N. we had this night a few drops of rain. 14. Dripping at intervals: the dust laid. 15. A wet day. The vulgar notion, that rain on this day (by the Popish calendar given to St. Swithin) is followed by the same daily for forty days, if tried at any one station in this part of the island will be found fallacious. There is in perhaps a majority of seasons, a general tendency to rain during this period, which in Ireland and on the western coasts of Britain may, in some, produce the effect in question: and the prejudice hence arising may have travelled into a climate where it does not hold good. 16. Thunder p. m. during a shower. 17. A slight shower p. m. dew on the grass. 18. A fine day: the Cumulostratus prevailed and the evening was very clear, with dew. 19. Showers.

## RESULTS.

Prevailing winds easterly to the new moon, afterwards westerly.

Barometer: greatest height 30·18 in.; least 29·54 in.

Mean of the period 29·875 in.

Thermometer: greatest height 79°; least 41°;

Mean of the period, 61·69°.

Evaporation 2·72 in. } in 28 days.
Rain 3·04 in. }

L. HOWARD.

TOTTENHAM,
*Seventh Month*, 19, 1815.

## III.

*Observations on the fall of Stones from the Air, or Aerolites.* *By M.* Marcel de Serres.

*Concluded from p. 250, Vol. XXXV.*

Showers of oily matter.

IT appears in short, that the difference observed between the showers of fire, and those of an oily matter which have been seen a great number of times, is only founded on the circumstance, that in the former this matter is in a state of phosphorescence, which has not taken place in the others. After these singular showers we may place those which are of a mucilaginous nature, and which by the report of Muschenbroek fell in Ireland in 1695. As chemistry teaches us that mucilage approaches to the nature of sugar or honey, we must class these along with the showers of honey, which cannot readily be admitted as the excretions of plants, as some philosophers have pretended. Silbershlag collected some of the matter which had been left by one of these showers, and saw that the paper on which it had fallen was covered with a viscid and thick liquor*. One of these dews very lately fell at Ulm, and is mentioned by all the Journalists, and it was so abundant that all the bodies which had been exposed to it were covered with a thick viscid matter, which was also found covering the surface of stagnant waters and springs.

Of mucilage: and of honey.

Showers of sulphur.

It might perhaps be presumed, that the matter which produces the globes of fire falls in the form of a shower under certain circumstances, and in the same manner as the matter of the aerolites is precipitated in a very divided state in showers of sulphur, of sand, and those which have been erroneously called showers of blood†.

* In his work already referred to.

† See Pliny's Histor. II. 56.—Mem. Acad. des Inscriptions Annee 1717—Lemair, Antiquités d'Orleans.

The

The showers of sulphur*, have been ascribed to a vegetable origin, though the facts will scarcely agree with this explanation. For the sulphureous shower which fell in Copenhagen in 1646, was accompanied by a heavy rain, while the air was infected by a smell of sulphur, and the sulphur which Wormius and other philosophers collected, had exactly the same qualities as that which is usually obtained from minerals. A shower of the same description again took place at Copenhagen in 1665†, which was preceded by a violent storm. The substance it brought with it, on being collected and thrown in the fire, produced a very evident smell of sulphur, and with the spirit of turpentine it formed a kind of balsam of sulphur. And very lately the shower which fell at Rastadt in 1801, was so very sulphureous that the substance was made use of in making matches‡. But the substance which in general accompanies these rains, resembles balsam of sulphur much more than sulphur itself. This was observed at Chatillon sur Seine, where the rain left a residue which was very fetid, thick, and adhesive§, and lastly in Ireland in 1695, where the matter that fell was of a deep yellow colour, and of a gluey consistence, with a very strong and disagreeable smell‖. This likewise possessed the property of being deliquescent in the air, and of drying by the action of caloric. Showers of the same kind have also been observed in the duchies of Mansfeld in 1658 and of Brunswick in 1721. Showers of sulphur.

It is equally absurd to consider the mineral showers, as produced from an animal origin, because that in some of them have been supposed to be found the excretions of butter-flies. Nor can we scarcely call in question the mineral showers which Showers of red matter.

* Moses, Spangenberg, Olaus-Wormius, Seigesbeck, and after them Muschenbroek mention these showers of sulphur, see tom ii. des Elémens de Physique de Muschenbroek.

† His work already referred to.

‡ Esprit des Journaux, Juellet, 1801.

§ Histoire naturelle de l'Air par Richard, tom. v.

‖ Muschenbroek and Izarn.

fell

fell in Wesphalia in 1543*, at Lowen in 1560, and at Embden in 1571. The latter was so extensive, that every thing exposed to the air was stained red within the circumference of ten or twelve leagues. It is also related, that in the year 1653, in Zealand, a rain of the same description dyed every thing red, and lastly that at Brussels, in 1646, a violent rain fell on a sudden, and all the water that was collected was very decidedly tinged with red†. It was even remarked, that all the rivers which flowed near that town had after eight hours, assumed the same colour. This rain was at first purple; its colour changed by degrees to yellow; and its taste was sourish like the waters of spa, which appeared to indicate the presence of carbonate of iron, a substance which undoubtedly forms an essential part of it. This rain, as well as that which fell at Ulm in 1755, were chemically examined‡.

Showers of red matter, &c.

At this period, about the end of the year 1755, showers of the same kind took place in Russia, Swabia, near the lake of Constance, and at Lucania in upper Italy. The heavens were

* Uber Wunderregen. Ulm 1755. This work contains a minute history of all the rains of this kind that have hitherto been known up to the present time. In it are also found the analysis of these showers.

† This water which was collected had a sourish taste, nearly the same as the spa water. On a small quantity of vinegar being poured to it, it formed a thick red precipitate. When kept for some time in a vessel that was well closed, the water of itself became turbid, and precipitated a viscid matter of a purplish colour, with some whitish flakes. On distilling this water a liquor was obtained of a sharp bitter taste. The taste and smell of the residue resembled that which is afforded by turf, and was thought, by those who examined it, to indicate the existence of organic substances. This shower lasted for near eight hours, its colour was a deep red when it began to fall, but became paler as this singular phenomenon approached its conclusion. See the work before cited.

‡ This shower afforded results similar to those of the shower which fell at Brussels on the 6th of October, 1646. The taste of the water was constantly sub-acid. The residue was of a deep red inclining to black, and in part attracted by the magnet which indicated the presence of iron. See Uber Wunderregen. Ulm 1755.

obscured

obscured during this rain at Lucania; and the atmosphere became quite red before it fell; the residue which it left was also reddish and had an earthy appearance. This rain was almost as thick and had the consistency of snow, as was likewise that which fell on the mountains of Plaisance on the 17th of January, 1810*. The latter, which was observed by a great number of persons, appeared at first white, but after several claps of thunder it became red, and at last returned to the white colour. In certain places it appeared of a flesh colour, while in others it was of a deep red; but it has always retained its colour after having been fused; this fact seems to prove, that it is with little probability the colour of these showers has been attributed to a varied or chatoyant reflection, similar to that of Mica, as some philosophers have pretended.

The testimony of too many persons concur in proof of the existence of showers of sand, to allow of their reality being denied. A shower of this kind was observed at Bagdad about the year 930†, and a long time before it fell, the sky appeared covered by a red cloud from whence was precipitated an immense quantity of reddish sand, differing totally from the sands which are found in that country. Some authors consider this sand as a ferruginous oxide: whatever it may be, the reality of this phenomenon is no less certain than that of the ferruginous shower which was seen on the Atlantic ocean in 1719‡, in latitude 45°, and longitude 32°, at the distance of about five or six leagues from the continent. This shower was preceded by a considerable degree of illumination, and lasted for more than nine hours without the air being disturbed§.

Showers of sand, &c.

* Litteratur Zeitung Jahr, 1812.

† Memoires sur l'Égypt, par M. Quatremere.

‡ Histoire naturelle de l'Air, par Richard, tom. V—.Id. Lithologie atmospherique, par. M. Izarn.

§ Père Feuillée exhibited some samples of this sand at the Academy of Sciences. As this sand was of a nature similar to that of the neighbouring shore, it was presumed it might have been conveyed by means of a water spout.

The

If the extraordinary showers be atmospheric, the aerolites are probably so too.

The various descriptions of residues or precipitates which we have been speaking of, seem to afford additional proofs of the atmospheric origin of aerolites. In fact, it is impossible and almost absurd to attribute all these showers and globes of fire to the irruptions of volcanos in the moon, or to portions of planetary matter. If, therefore, we are obliged to admit of their atmospheric origin, it is scarcely possible to avoid doing the same with regard to aerolites, since these meteors, whatever may be their name, pass so insensibly from one to the other, and have so much resemblance, that the cause attributed to the former of these phenomena cannot be rejected for the latter.

Difficulties as to the theory.

As to the difficulties which we have here concisely detailed, they have been long ago known, and if the atmospheric origin of aerolites has not been admitted as the most probable, it is because some very specious objections have appeared and sufficiently well founded to oppose this theory. We must confess the formation and fall of aerolites is a phenomena so singular, and so different from all those of which we are able to trace the progress and effect, that it is much easier to attack a theory which shall endeavour to account for them, than to defend that which appears to have the greatest probability.

Arguments respecting the weight and mass of aerolites, and their having been formed by condensation.

The strongest objection advanced against the atmospheric origin of aerolites, is founded on their compactness and weight, which seem plainly to shew how difficult it is to conceive the formation of bodies, of such a size and weight, in the atmosphere: How, it may be said, is it possible that particles, so much heavier than the air, can rise to those elevated regions where the meteorolites appear, and that these metallic particles should remain suspended, in the state of vapor, in the atmosphere, till they assume the form of globes, or at last unite into a mass of a certain volume. It may, nevertheless, be remarked, in this instance, that the particles which compose the globes of fire, and which no philosopher has ever pretended come from the moon, must likewise have been in the shape of vapour previous to their being afterwards condensed into masses of gelatine, which are sometimes of a very considerable size. It is necessary to mention, as a proof, the gelatinous mass

mass which fell at Groepzig in Saxony, and was more than five feet in length, and equal in size to the body of a man. For it is not less difficult to admit, that these globes can be formed in the air, than the aerolites themselves. If we endeavour to combine all these facts, there is sufficient inducement to think, that a kind of formation of metals may take place in the air in the same manner as we see the plants, and organic bodies in general, have the faculty of changing the nature of the substances they have absorbed*. It is well known, that hydrogen gas, at a certain temperature, can volatilize some of the metals; but again it is hardly possible that this gas should have given the form of vapour to the metallic particles of aerolites, since that it is found in the air in a quantity altogether inconsiderable. Besides which, hydrogen† never volatilises either nichel or iron, nor the various earthy substances which enter into the composition of these stones. Nor can heat give these

* The experiments which have been made on the formation of the metals and earths in plants cultivated in sulphur or in charcoal, and watered for two months with distilled water, may be found, 1st. in the memoir of Schrader, which obtained the prize of the Academy of Berlin. An extract may be seen in the Journal de Chimie de Gehlen, vol. II. 2d. In the memoir of Crell, entitled, *Pericula genesin carbonis puri, quem vocant, &c. in plantis vegetantibus investigantia.* This memoir was read to the Society of Gottengen, and has been published as an extract in the Journal de Medecine de Salzbourg, of the 8th of April, 1811. The experiments of Schrader and of Crell were made with the most scrupulous exactness.

† Scheele is the first who made the remark, that arsenic unites with gaseous hydrogen. See his Essays, German edition, tom. II, p. 136. Potassium is dissolved in hydrogen. M. Berthollet, Introduction to Thompson's Chemistry. A current of hydrogen gas, or of azotic gas, determines the volatilization of sodium. Recherches physiques et chemiques, par MM. Gay Lussac et Thenard, tom. II, p. 241. Ritter has even asserted, that a great number of metals may be combined with hydrogen gas by the action of a strong galvanic pile, but this has not been proved. See Annalen der Physik, Von Gilbert. Much information may also be had from the observations of M. Corradori, published in the Journal of Brugnatelli.

substances

substances the form of vapour; and even then their composition, which, in general, is nearly the same, would not be easily accounted for. Lastly, how can these vapours, floating in the air, arrange themselves so as to form so compounded a mixture. And, added to that, the regular form of the metallic parts indicate a kind of fusion, which, as M. Proust remarks, is not consistent with their slight degree of oxidation.

The metallic body which falls is considered as the residue of a much larger mass of combustible matter.

Admitting, that during the ignition of the metallic nucleus, all these metallic parts are formed, the small magnitude of the stone itself may then be accounted for in comparison with the immense size of the globe of fire from which it proceeds. In fact, the metallic stone is nothing else but the residue, or, as it may be called, the *caput mortuum* remaining after the great combustion; and the adhesive substance resembles pitch which is found round some aerolites, may be considered as part of the substances which has not been on fire. If the matter of which these globes of fire that precede the fall of aerolites are composed, were not extremely combustible, the globes themselves would not be so extensive. nor their ignition of so long a duration. The meteorolites themselves do not contain any very inflammable substances, because these were consumed previous to the fall of these bodies. It is also to be observed, that phosphate of iron is sometimes spread over the aerolites, as was seen in that which fell in Russia in the year 1807. And,

The meteorolites resemble, in their component parts, the bog ore of iron.

as M. Vauquelin has remarked*, nothing more is required, in the bog ores of iron, but nickel, in order that they may resemble, in composition, the atmospheric stones. As these ores are formed almost always in the midst of marshes, we may

* This able chemist, on analysing five species of the bog ore of iron, discovered that they are all composed of the same principles, which are silex, alumine, lime, oxide of manganese, phosphoric acid, magnesia, and the chromic acid. As M. Vauquelin took these ores without selection, he considered that, very probably, all the ores of the same kind contain the same substances; and lastly, that nickel only is wanting in the composition of these ores to complete their resemblance with the atmospheric stones. Annales du Museum d'histoire naturelle, tom. VIII, p. 459.

there-

there see a kind of slow formation of aerolites. The facts we have already related sufficiently explain why, under several circumstances, the cloud precedes the fall of the aerolites; for it contains all the substances of which they are composed, and, in this view, the phenomenon may, in a certain respect, be compared with the solution of salts.

It appears, lastly, that, by supposing the power which supports these globes of fire as resulting from the inflammation or the formation of vapours, we do not admit of an unfounded hypothesis. For, in fact, whenever these globes burn with little energy, the matter of which they are composed falls very soon, while they rise again as soon as ever the inflammation is renewed. This is also the case in our rockets, where the force of ignition raises and sustains masses of a considerable weight. This force likewise employs its action on the course of the aerolites; and, as it is opposed to their weight, it obliges the stone to follow an intermediate direction between the two impulses it receives. Observation proves, that aerolites fall in proportion as the fire is extinguished, and that when, as at Connecticut, the combustion is again increased and accompanied by explosions, the stone rises in the air again.

The projectile force of aerolites is derived from their combustion.

From these facts it may be imagined, why this class of phenomena is so frequent in the warmest months, and so rare in winter; and why they appear in the evening very frequently accompanied by storms. The causes of these phenomena, though in appearance so different, have, nevertheless, some relation to each other. Rain, for example, is only the effect of a precipitation of the water which is continually rising in the air, and aerolites probably depend only upon the precipitation of a great number of substances which are continually evaporating, and of which the reaction on each other may form new combinations. This hypothesis will not appear unfounded if we attend to the immense quantity of compound substances which organic bodies, stagnant waters, and all bodies which are in a state of decomposition, incessantly exhale, and are lost in the air without its being known what afterwards becomes of them. It is very natural, therefore, to inquire what means nature

Why these falling stones depend on seasons and climates. They resemble common rain in some respects.

has to counterbalance this constant evaporation, and to purify the atmosphere which contains all these volatilised matters. It it probable that nature employs other methods to purify the air; and it is most likely, that organized bodies are the most powerful agents employed in this work. Plants especially seem to be charged with this office; they even appear to absorb mucilaginous matter, which has been proved by MM. Duputryen, Thenard, and Mocate to exist in the air in a considerably large quantity. Vegetables are nourished by it; and this cause, together with several others, makes it possible to conceive how that plants set in matter which is incapable of supplying them with any alimentary moisture, should nevertheless vegetate and grow with strength.

Retrospect. Such are the principal proofs, or rather the most decisive facts, which give probability to the hypothesis, which considers aerolites as formed in our atmosphere. We can even assert, that these proofs are sufficiently strong to render this opinion deserving of more attentive examination; and we are far from thinking with M. Bigot de Morogues, that this supposition itself is in any respect hasty or rash. With equal frankness it must be allowed, that the hypothesis of which we have been giving an account, is subject to a number of objections*; but can it be proved as has been advanced, in my opinion, rather gratuitously by M. Bigot de Morogues, that aerolites have been

* One of the strongest objections against the hypothesis, which admits the formation of aerolites in the midst of terrestrial regions, is the total absence of oxygen in the stone which fell at Lissa, and was analysed by Klaproth. It is in fact singular, that the particles of iron and martial pyrites should have been able to resist even a short inflammation without beginning to be oxided. But in the aerolites such as that which fell at Alais, the charcoal which they contained continued to burn, and the silex which was separated from it, was not in the state of jelly as in the other meteorolites, which indicates that they had not experienced a great degree of heat. This last fact is unfavourable to the opinion, which considers aerolites as stones shot from the volcanoes of the moon. See the analysis of a stone that fell at Alais the 15th of March, 1806, by M. Thenard; Annales de Chimie. Année, 1806. p. 103.

small

small celestial bodies. If this opinion advanced by the most skilful geometricians, could be demonstrated, it certainly would be absurd to attempt by suppositions to explain a fact so firmly established. But it cannot be supposed we have been led into an error of this kind; for in all the explanations hitherto given of this most singular phenomenon, there has not been the least evidence which can be considered as amounting to demonstrative proof.

The observations I have here made, respecting M. Bigot de Morogues, does not prevent me from admitting, that his work, which is the latest we have on aerolites*, is in general written with that reserve which so delicate a subject requires. His book is indeed nearly complete on this interesting subject: it is nevertheless to be wished, that the author had been more disposed to make us acquainted with the opinions that have been advanced by foreign philosophers respecting aerolites, especially those of the English and German naturalists. But these omissions respecting which it may be allowed to attach blame to M. Bigot de Morogues, and even arise from the extreme attention he has given to his work, are unfortunately too common in the greatest part of the books published at present in France. This neglect of making ourselves acquainted with the works of learned foreigners, is the more to be regretted, as every branch of the sciences is at present cultivated with success in England, Germany, and Italy. This conduct renders it impossible for us to avoid regretting the time when the Latin was the only language employed in the sciences, when consequently it did not require so much study for nations to understand each other. In this point of view the authority of Paracelsus† and of Bernard de Pallisy‡, who first published in the

* It appeared in 1812, and is sold by Merlin, Quai des Augustins, No. 29.

† The most ancient work of Paracelsus is a treatise on Medicine; entitled, *Nützliche Bücher von der Franzoesichen Kranckheit*, and appeared at Nuremberg, in 1565.

‡ An abridgement of the Agriculture of Bernard de Palissy was published in 1504. His treatise on Marl, and that on the nature of rivers and fountains in 1580.

the vulgar tongue, and that of Belon* and of Ambrose Paré†, who were also amongst the first who abandoned the Latin language in their writings‡, has had a fatal influence of which the sciences will have still more reason to complain. M. Bigot de Morogues seems to have been very fortunate in his idea of dividing his book into sections, each of which relate to the epocha when such or such an opinion was prevalent. And he shews us how much, until the sixth epocha, the public opinion

* Belon's history of the Nature of Birds appeared in 1555; but in 1553 he had already given the public his observations made in Greece.

† We have a book of Ambrose Paré, on Anatomical Administration, which appeared in 1549. As to his treatise on wounds, made by Arquebuses, it was not brought out until 1651.

‡ These writers were not the only ones who abandoned the use of the Latin language; for from the middle of the XVIth century this custom began to be almost universal: the surgeons especially set the example; and as early as 1570, there were already five books on surgery in the vulgar tongue. Three were published in Germany; the most ancient by Karethanus in 1479, the second by Hermenius Ryff in 1541; and the third by Paracelsus in 1585. In Italy that of Rostini was published in 1557, and in France Dalechamp's appeared in 1570. The German Mettinger was one of the first who wrote on medicine in his native language, and his work entitled: *Regimen der Jungen Kinder* was printed at Vienna in 1474. Later towards 1552, there appeared in France the works of Thierry de Héry on Medecine. And before the anatomy of Ambrose Paré, which dates in 1549, Hermenius Ryff had published in 1541, an Anatomical Description of all the parts of the Human Body. (*Der menschen Vahrhaftige beschreibung oder Anatomy.*) The histories of anatomy also written in the vulgar tongue by John Hall, and John Banister, are posterior, for they date one in the year 1561 and the other in 1578. The facility with which a person writes in his native language, joined to the example such very different authors had given, as Luther in his celebrated theses which he published in 1516, Rabelais in his entertaining fictions towards 1551; and later the inimitable Montaigne in 1583, rendered the Latin language so much neglected, that, towards the latter end of the XVIth age, a number of writers had entirely abandoned it in their works. Olivier de Serres was one of the first amongst us who by the grace of his style, perhaps, contributed the most to make the French tongue be adopted as the language of science.

had

had varied respecting the reality of the phenomenon of aerolites, and to what a degree in early times the wonderful descriptions of the fall of stones had been exaggerated by superstition, and were received and frequently considered as religious mysteries. When the sciences began to flourish again, philosophers were so much prejudiced against phenomena which appeared to them to accord so little with the laws of nature, that they disdained paying any attention to them, whilst the historians were eager to register in their annals a fact, to which the Emperor himself was a witness*. But in an age when every thing that could not be explained by reason, passed for an invention created by superstition, it was found that learned men endeavoured, by the most specious reasoning, to annihilate the reality of a fact, to which the authority of ages could not induce them to admit, because they were unable to conceive the possibility of its happening. However, in the midst of these disputes which the great name of Gassendi† was unable to terminate, a great quantity of stones fell at Lucé in 1768, in the centre even of France; and notwithstanding this fact, which was clearly verified by evidence, the Academy of Sciences persisted in considering it as one of those popular prejudices which were beneath the attention of natural philosophers. The stones which afterwards fell in India, however, attracted the attention of the learned, but did not overcome all the prejudices, and it required, in short, the great number of aerolites which fell at l'Aigle, and at the gates of Paris, in order to confirm the reality of this singular phenomenon. Since that epocha, which goes no further back than 1803, the observations have been so multiplied, that there is perhaps at this day no fact better ascertained; of course a doubt is no longer admitted. This phenomenon is even so usual and frequent, that in seeing

* On the 7th of November, 1492, in the neighbourhood of Ensisheim, an aerolite fell near that prince at the moment when at the head of his troops he was going to give battle to the French army.

† Gassendi gives an account of an aerolite which fell on the 27th of November, 1627, on Mount Vaiser in Provence.

them renewed at periods that are so little distant, we are still more induced to believe that aerolites are formed in our atmosphere. Whatever may be the fact, it is natural to think, after the rapid sketch first traced, that the history of aerolites is connected with that of our prejudices and errors, and that it even belongs to the history of the world.

---

## IV.

### *The Seeds of all Plants first formed in the Roots, shewn in a Letter from Mrs.* AGNES IBBETSON.

*To Mr. Nicholson.*

SIR,

The subject of the letter.

IN my last, I merely announced the discovery I had made of the astonishing fact, that the seeds of all plants were formed in the root only. I shall now endeavour to evince the truth of this assertion by shewing the manner in which they are first protruded in the radicle, their progress through the root, their passage up the alburnum vessels in the stem, and their entrance into the pericarp, while the buds are still in their *collected state.* I shall also shew the necessity of never admitting any part to belong to a plant which cannot be traced in all its different stages, and many other matters of equal importance to the elucidation of a fact of such extreme consequence to phytology, that, *if established,* it will fix the root as the laboratory of plants, shew where we must seek the source of its growth, the cause of most of its disorders, and their radical cure; give us a key to the general formation of vegetables of every kind; and, in short, teach us (what we are certainly wholly ignorant of) *what a plant is.*

Many years discovering the seeds.

I mentioned, in my former letter, how many years I have been seeking this fact, most difficult to discover, because of the very short time the seeds are to be seen in the root, and mounting the alburnum. From watching and cutting the radicles

dicles in every season of their growth, there appears only one time in the year when they are fitted to produce the seeds; they are then quite loaded with moisture, and, if laid open with care, will be found inflated in several parts, in which the seeds, (surrounded with alburnum) *are imbedded.* Just before the barking season, these minor roots increase greatly, but do not swell into protuberances, as when the seeds are forming. Their doing so must, I think, be owing to the sap being retained rather longer than usual for their formation; for, in every instance with which I am acquainted, where alburnum is produced, it is effected by the momentary stoppage of the sap, which then agglutinates and produces a sort of jelly: not that I suppose the seeds to be composed of alburnum *only*; even at first the eye can discover a difference, and much alteration is produced, in the first meeting and coalescing with the natural juices of the plant. The seeds are extremely diminutive when first protruded, and hardly to be discovered in any microscope except the solar; but just before they leave the radicle, and pour into the root, a common treble eye-glass can, (if united) shew them plainly. All the radicles contract at the part where they join the root. It is in this place the seeds are best discovered, in the narrow pass where, crowded together, *numbers* make them *so conspicuous*. Formed of the freshest juices of the earth, which we call *sap*. just entering the plant, and producing a new compound with its liquid, the first formation of the seed must be simple; but how different its future progress—how various the chemical changes—how astonishing the innumerable solutions that must be added to complete this extraordinary production. I have often, with amazement, viewed, and endeavoured to count, the number of vessels (changing almost every day) required to complete a seed, from the time it appears in a bed till it is fit for replacing in the earth, and capable of forming the embryo. Yet even these changes are not to be compared to the variety bestowed on its earlier existence, to the innumerable chemical combinations which must take place in the seed, and around it, when passing from the root to the stem, in that part (between the bark and wood) where it must equally

Formation of the seeds in the root.

Juices which compose the seed.

partake of the blood of the plant, the sap, the juices of the line of life, and nectary, of the curious compounds of the fruit, as well as those juices peculiarly formed for the seed: in short, it must truly be a most wonderful production.

Formation of the line of life on which they hang.

I have shewn, that the radicle, at the time it produces the seeds, swells into protuberances; the line is also formed at the same place, but so confused is the *mass*, and so diminutive the objects, though, from its consequences you may be sure that it is here the line developes and pushes up, yet it is not actually to be seen to grow, till the seeds appear hanging on their stalks, and forcing their way through the contracted space which conveys them to the root: but the vessels (supported by each other) are constantly moving upwards for two months preceding the flowering of the plant. When I first discovered the seeds in the radicle, I concluded that they were carried up the tree by the rush of the sap up the vacuum caused in the passage of the alburnum by the retiring bark: but I was soon undeceived, and I find no projectile force is required for the purpose. The vessels round which the seeds hang, increasing at the point of the radicle where they are first formed, are continually forcing themselves up the passage, till, in various different shoots, they attain their proper situation in the bark, lengthening and developing, and requiring no other means to gain the seed-vessels, than growing quickly up the alburnum cylinder.

Early discovery of the seeds.

Most phytologists have endeavoured to trace the first growth of the seed; Duhamel mentions, with astonishment, his opening the pericarp in the first bud when the flowers were still in their aggregate state, and finding the seeds within it. I have traced them even sooner, and could not then imagine where they could first be produced. The present discovery of the seeds in the roots enlightens the whole subject, and how greatly does it add to the beauty of the contrivance, the embellishment of the design, when, in the cold season of the year, all the important parts of the tree or shrub are forming; where the protection of the earth gives them warmth and support; when, without this management, all the time of winter must be lost to nature, instead of which it is, (though without appearing

appearing so) the *busiest season of the year*; when the seeds are formed, the pollen protruded, the flower bud-engendered; no time is lost, no part of the plant left idle or dormant. It has been mentioned, with astonishment, by many, that the bud should, the moment it opened, be so tender, and yet so hardy, when closed, as to sustain itself through the winter in all the varieties of climate. But it may well do so; for it is merely the *house*; the *inhabitant* is in a *warmer* and *safer* place; while the bud is the habitation only; preparing, with the greatest care, for the entrance of that being which is too susceptible to support the winter unguarded. I must not, however, be misunderstood; the *leaf-bud* is, (as I have always shewn,) very different from the flower-bud, and formed in the interior, as well as in the exterior nearly at the spot where it is protruded; bnt the flower-bud is of a different nature; its arising from the line of life is sufficient to announce the delicacy and importance of its formation, as that* vessel is the origin of all the essential and important parts of a plant. Though the greatest pains have been taken to prevent the seeds, as they pass through the root, from being overpowered by the juices of the inner bark, which would probably greatly endanger and debilitate them; and that this contrivance is merely adding a few rows of wood between the bark and alburnum, yet the seeds must, I should suppose, partake of the blood of the plant, though saved from immersion in it, which they would certainly receive if not protected.

Safety of the interior of the bud.

As I have now, as nearly as possible, shewn the first formation of the seeds, I shall next describe the disposition of the parts that form the peculiar sort of radicle which contains them; for it is only in the larger ones the seeds are protruded; they are never discovered in the threads. There are rarely, therefore, above six or seven of these roots to each tree. The interior of this radicle (which should have an appropriate name) is

Difference of the leaf-bud.

* What I have always called the line of life, that cylinder which sheathes the pith, is known by the French Botanists as "l'etui tubulaire," or medulary canal.

is divided into three parts, the *middle* being the largest, and forming a cylinder, through which the sap flows continually, while the two sides are dedicated to the creating the seeds: these sort of roots generally run in a horizontal direction, and many of them surround the higher part of the tap root, and thus lay up in the second root, *that matter* to be used in any emergency, or when the radicles are renewing; the seeds on quitting the alburnum in the root, enter the same vessel in the stem: it is well known, that this cylinder lines the interior of the bark, forming a sort of intervening part, which is alternately liquid and jelly, but which in both cases, is contained in vessels surrounded by a twisted exterior, within which the seeds pass tied to a *thread* of the line of life; there being five or six of these stems within each vessel. My mentioning the single vessel, and then the cylinder may make a confusion in the explanation of the form of the alburnum, as well as of the line of life; to avoid which, I shall here explain the meaning of both: both are cylinders surrounding the stem in their appropriate places; both are divided into separate vessels, each containing their allotted ingredients, and, therefore, may be mentioned as the cylinder composed of such a quantity of vessels, or as one of the aggregate number. They soon reach from the bottom to the top of the tree, and the seeds are fastened on them as currants on a stalk, or as grapes on a vine; for they have all their peculiar arrangements according to the form they present in the pericarp; the line with its accompanying seeds, running up in a few, or many divisions; and that line or vessel, is the one through which the joint juices of the stamen and pistil afterwards convey the matter to impregnate the seeds.

The seed radicles are but few.

Form of the cylinders of the alburnum, and the line of life.

When the seeds are passing from the root to the stem, they appear in very great numbers, and are very conspicuous, causing much variation in the interior; in a young plant, the undulating form within, is even marked in the hardened kind at this time, and makes the constant motion very conspicuous. But to see this perfectly the tree must not be cut; it is only in tearing one, that in splitting it open from the root to the higher branches, it displays itself; then the change of each part is most plain

How the seeds hang on their stalks.

Proper to tear the tree open.

plain, and the effect of the mounting of the seed, *most evident.* The wood retires to leave room for the alburnum vessels, whenever any new branch produces flowers; and will, therefore, require space for the seeds to crowd round the buds, while the bark also recedes outwardly and forms those undulations just mentioned. A blow or cut to the tree at this time, is most hurtful, and will turn all the seeds black at the side which receives it; when arrived at the situation where the flowers are collected, the most wonderful part of the process begins. The large assemblage of the seeds at each side of the stem (if divided where the general flower stalk first shoots) is really as wonderful a scene as nature presents. The seeds pour into the pericarp when this stalk is not an inch in length, while the pollen is at the same time preparing also to enter its cases. That they should neither of them intermix or confuse each other, though occupying so small a space, that the eye can hardly discern it without magnifiers, and yet that they should remain as completely separate as if divided with strong partitions, is really astonishing; especially as the alburnum vessels swell so considerably as to push back both bark and wood, and fill up every space but the *middle*, which is occupied by the pollen: on comparing the seeds in the root, with those entering the seed-vessels in the same plant, that is, taking *a single seed* from each part, *at one time*; I found they differed not the least in *size* or *shape*, though extremely magnified; and that they *grow* not, therefore, as they *rise up the tree.* My general mode of proceeding with such diminutive specimens, is to immerse them in a bubble of glass; but the seeds being too tender and delicate to bear even so momentary a heat, I now substituted water in its stead. It is certain that they magnify *much more within the drop*, than if placed at the *focus.* I should suppose that the front of the circle, serves as a concave mirror to the object to be viewed; but it will not act well, if the specimen is too opake, but being merely the corculum of the seeds, nothing can be more clear and transparent, and any person that has beheld them in a very young bud, would recognise the objects again; but there are so many deceptions in

Neither pollen nor seeds intermix.

Proper to place the specimen within a bubble or drop.

*It is so. See Martin's optics.*

in a plant, that I should never (in spite of this resemblance) have acknowledged them *to be seeds*, if I had not in innumerable instances traced them from the radicle to the root, and then to the flower, and seen them pass into the seed vessels while under my eye in the microscope, After such absolute proof, I could not doubt their identity. But the manner in which the seeds pass into the pericarp, within the various buds, is much to be admired; in some trees (that is in all firs) they collect at the bottom of the flower-stalk, and form an heap; here they remain for some time; and as the alburnum vessels had long before made a dip to each flower, when it conveyed the bud to its cradle in the bark, the line of life accompanying it, its passage is already prepared; as it is through these *same alburnum vessels* it passes from the aggregate numbers, dropping a few seeds into each seed vessel, which gapes wide to receive them; and so exact is the calculation, that by the time the buds have each attained their proper number, the heap has disappeared, and except a few lingering ones in the vessel which conveys them, all will have past away. (See the branch of Larch in Journal, No. 161. p. 1.) But in other trees there are no collection at the bottom of the flower-stalk; the seeds pass at once from the alburnum in the stem, to the same vessel in the stalk; dropping such a number of seeds into each paricarp, which opens to receive them, and immediately closes on its treasure. In trees, I have never been able to catch the seeds passing into the pericarp while under my eye; that is, move during the moment of observation; though I have cut down many a tree and prepared the specimen within the hour; but in plants that rise each year from the earth, I have not only seen it frequently myself, but shewn it to others while moving; particularly the strawberry root, where the seeds are very large and very conspicuous; and in the arum, where if properly dissected and retained in the right cylinder, they may be seen mounting in the vessels, and removing into the various pericarps for nearly a quarter of an hour; some plants are stronger in their motions and preserve it longer than others; for as it is wholly caused by the mechanical force of the

Seeds collect at the bottom of the flower stalk.

No collection at the bottom of the flower stalk

The seeds moving into the pericarp.

the spiral wire, and that in this respect it may be compared to the muscles of an animal, which can be animated by the galvanic trough a *certain time after death*; so does the spiral wire retain its vital energy for a little time, longer, or shorter, according to the form of the plant, and preserve its mechanical vigour after the line has ceased to exist. To see the effect of this mechanism in plants, I always prefer the most common, provided they are well-nourished and healthy. Exotics will often produce more extraordinary and elegant contrivances, but the plants of the country have most vigour, and their mechanism is, in general, in admirable order; there is a great difference in the operations performed on an exotic, and a native. The pistil and stamen do not accommodate each other in the former, as in the latter; the spiral wire does not contract or dilate with that vigour; the wood does no move with equal energy, or the gatherers turn with equal force; and there is no part in which the mechanism is so much excited as in the throwing or moving up of the seeds in the alburnum vessel; it appears to be the grand effort in which all its energies are engaged; every part of the plant conduces to this effect, and lends its assistance to its completion. I am almost angry with myself, when I think how many things would have suggested the discovery of the seeds in the root, that I puzzled about it so long in endeavouring to find their origin; since the leopard lilies which form their seeds up the stem, and the *crinums* which collect them so far down the stalk, should have taught me, that the pericarp and flower were not necessary to their formation*.

To shew the mechanism prefer common plants.

I was in hopes of shewing how the pollen passes up the stem, which I did not do in my last, though I have now discovered that in trees and shrubs it is protruded in the tap-root,

**Respecting the formation of the pollen.**

* It is a very common thing, particularly in the firs, to find a single cone growing out at the bottom of the tree, unattended with any branch or leaves: what a proof this is, of the seeds coming from the root, when a single collection has been able to pierce through bark and rind, and display themselves without further accompaniment.

and

conveyed through the common root: and that I have again detected it at the top of the stem where it enters the flower-stalk; yet I have not discovered the part in which it mounts; for the season closed on my labours, before I had time perfectly to complete my task. Indeed, in no science is it so necessary to continue *invariably* what has been undertaken; most studies may be pursued or laid by as industry or idleness *suggest*; but the phytologist must not lose a day, or the season flies, and he has to wait a whole year before he can renew his subject, or recover the lost appearance of the specimen he neglected to draw; so quickly also do the pictures succeed each other in dissecting. Each separate part, whether of the bark, wood, or pith, is so divided into innumerable cylinders, made to approach or fall back; between which are admitted the different ingredients of the plant, beginning at the interior of the root, and so exactly arranged according to the ensuing season, that what has been discovered one month will appear to be contradicted the next; from the total change which will have taken place *within*; if, therefore, the dissecter has not already learnt, that the seasons are to the full as varying in the interior of the plant as without, how puzzled he must be; it is only by tracing the repeated changes for many succeeding years, that I have been enabled to lay before the public these various discoveries. How difficult then, how *impossible* for a person who dissects but a few times in a year, to understand the plan and formation of a vegetable.

Alterations within the stem.

I shall now shew the necessity of never admitting any *part to belong to a plant*, that cannot be traced in all its different stages. There is a source of deception so likely to inveigle a phytologist (and which was, in truth, the cause of delaying the discovery now so happily effected) of "the growth of seeds in the root." I mean the quantity of criptogamia discovered between the rind and bark in every tree or shrub. I shall scarcely be credited when I say, that extremely minute festoons of flowers, apparent branches of fruit, and bunches of seeds, appear throughout the year, and are continually found between

Quantity of criptogamia between the cylinders.

between the various cylinders of the bark and rind*. The bark matter, instead of being confined in regular vessels, as is generally supposed, is found in masses which increase and congeal till the warmth of the summer again renders them liquid, and confines them within their proper cylinders. It is in these masses the criptogamia plants are formed; it is here they originate and nourish their roots; and these sprigs have, I doubt not, much puzzled others as well as myself. I have long been acquainted with them, from having first found them in the grasses and grains†. But when I first discovered the seeds in the alburnum, I concluded that it was a criptogamia, only more advanced within the interior; and as I had known the viscum album, and several others of the parasite plants, throw their roots even into the wood, it was natural I should suppose it a new plant of this kind not yet noticed; till, with astonishment, I traced it from part to part, and at last saw the seeds enter the buds; but there is a proof which completely draws a line between all that is formed within the plant, and appertains to it, and that which is a stranger only supported and nourished by it. In the first case, it always shoots perpendicularly; but in the latter, let it run ever so far between the cylinders, it always strikes out at last, and therefore shoots horizontally. This will, at least, serve to ascertain which are crip-

The masses of matter in the bark.

Parasite plants growing from them.

* Till I found the innumerable parasite plants between the folds or cylinders of the bark and rind, I never could account for that idea of Linnæus, that it required five years to complete the formation and evolutions of a branch; each year composing one part; and that they then came forth in their perfect state. I have no doubt that he was acquainted with these criptogamia I have described, and supposed that they belonged to the identical plant on which they were reared, and by which supported. I never followed an idea of this truly great man, that it did not lead to a discovery shewing his perfect knowledge of the forms of plants. His *mistakes* are often more valuable than the *knowledge* of others.

† In the wheat alone I have found four; and in a tree they are often known by the variously formed cuts that are made through the rind to let out the plant, or admit air to the seeds, the criptogamia being generally too small to be seen without strong magnifiers.

togamia

togamia plants, and to retain for the tree all that naturally belongs to it.

I find I have been accused, by gentlemen of the first ability, for not mixing philosophical trials with my specimens and descriptions, simply meant to illustrate the *formation, habit, and manner of plants.* As it was my original plan to proceed no further yet, and that I am rather proud of keeping to that resolution, when acting otherwise would be so pleasant, I shall now lay before the public my reasons for so doing. I have long been persuaded, that all experiments of the kind are perfectly *useless,* nay, highly *injurious,* to the science, till a thorough knowledge of the form, habit, and nature of a plant is obtained. How, indeed, could a surgeon judge of a wound he had made, and draw any just inference from its appearance, without a thorough *knowledge of anatomy,* and a perfect insight into the part *affected,* and of the vessels touched? A person performing trials of this kind on a tree, cuts into it without the smallest idea at the *time of doing it,* what his knife is passing through, or the danger of his destroying the rising productions that are travelling upwards, between the cylinders, to their several places of destination. I have said, that two months in the year the seeds are constantly mounting the tree; a little earlier the pollen is travelling also to the summit; and when this is not the case, the leaf-bud, forming at the pre-existing shoot, is sending up its leaf-stems through the new branch of the present year. All this must make so great a difference in cutting a tree, as to leave the person (who knows not of their existence, and yet is to judge of the consequences of the cut) in an absolute perplexity. A thorough acquaintance, also, with the vessels *(which are various)* is most necessary. The motion of the *wood* is so continual, that if, by certain signs, he has not learnt to know its direction, and appreciate the ultimate consequences of its change of place, how is he to proceed? No other way than by studying diligently, and waiting, with perfect patience, till that knowledge is acquired, *which will fit him for such a superior undertaking*; or he will be like a man who begins to build his house by the attics, throwing scaffolding from the neigh-

The different parts mounting the tree.

neighbouring houses to support this strange contrivance, never likely to produce a good foundation, or a well-established knowledge.

The nature of my first plan.

My first plan was to procure, by every possible means, a complete acquaintance with plants, both native and exotic; not of their *names* and *classes*, (though the latter is certainly of great consequence) but of their *form, nature,* and *habits*; their *vessels, mechanism,* and *juices*; and when that task is *complete,* to turn to the philosophical part. May I not hope, then, that the public will bear with my stupidity a little longer, and see the necessity of my shewing the beginning, before I venture on the latter part, of my work. And if I live not to finish my labours, (which is most probable) I still hope, that the little I have done may suggest to others the necessity of completing the foundation before a system is thought of, or a theory laid down.

I greatly regret, that the prints are now become much too large for insertion in your Journal, as they grow unintelligible when so much reduced, but when possible they shall always be added.

I am, Sir,
Your obliged, humble Servant,
AGNES IBBETSON.

---

## V.

*Remarkable Fact of the Glass of Windows being corroded by the Vapours from Copper Works. In a Letter from a Correspondent.*

*To Mr. Nicholson.*

SIR,

General effect of the vapours of copper works.

IN visiting the copper smelting works near this town, I was much struck with the barren appearance of the soil, and with the leafless state and deadness of the trees for a considerable distance round the works; but what excited my surprise the

the most, was the singular appearance of the glass in the window of a counting house attached to one of the works. The panes looked as if covered over with liquid starch, to answer the purpose of a blind, but, upon a nearer view, I found they had lost their polish, the surface exhibiting the appearance of glass corroded by fluoric acid. Upon mentioning this circumstance to a person residing near the spot, I was informed, that the same corroded appearance was to be observed in the windows of his own house, which was a quarter of a mile distant. Several substances have been applied to remove this misty appearance; wet red clay, rubbed on with the palm of the hand, has been found most successful; by long-continued friction with this substance, the rough surface is entirely removed, and the original polish restored.

Window glass corroded.

The copper ores that are usually worked here are brought from Cornwall. The fumes arising from roasting the ores, are copious and white, and continue a long time in the atmosphere where they are dissipated. Their smell is sulphureous and excites coughing.

I beg to be informed, through the medium of your excellent Journal, what it is in the smoke that can occasion such a singular appearance in the windows of the neighbouring houses.

I remain,

Sir,

Your obedient Servant,

VIATOR.

*Swansea,*
*August 23d,* 1813.

---

## VI.

*Cursory Remarks on the mineral Substance called, in Derbyshire, Rotten Stone*. By William Martin, F. L. S. &c.*

MR. KIRWAN, in his "Elements of Mineralogy," (vol. i. p. 203.) states, that Tripoli is often of pseudo-volca-

Rotten stone considered as of volcanic origin.

* Manchester Memoirs, VIII, (or II N. J.)

nic

nic and sometimes, perhaps, of genuine volcanic origin; he adds, however, that "it also frequently arises from the decomposition or disintegration of other stones." The latter observation appears to apply strictly to our Derbyshire *rotten stone**, which is usually considered by mineralogists as a *variety* of Tripoli, originating from some unknown decomposed stone of the argillaceous kind. That the substance producing *rotten stone* is, however, in its primary state, a *calcarious* and not an *argillaceous* stone, can only be doubted, I think, by those who have not had an opportunity of examing this fossil in its native repository. Indeed, I feel little hesitation in affirming, that the phenomena, attendant on the substance in question, strongly support the original idea of the late ingenious Mr. Whitehurst, who from personal and extensive observation, was led to conclude, that the parent rock of the Derbyshire rotten stone was *black marble*†, or some other variety of our dark-coloured lime-stones.

Account of the rotten stone pits on Bakewell Moor.

It is some years back since I availed myself of a favourable opportunity that occurred of examining the rotten-stone pits on Bakewell Moor‡, and which, I understand, are only opened at particular periods—that is, every third or fourth year, according to the demand which may then prevail for the fossil as an article of traffic. On looking over the *memoranda*, made at the time of visiting these pits, I find they differ, in some trifling respects, from Mr. Whitehurst's account of the mode in which rotten stone is procured, the appearances it exhibits as a mineral deposit, &c.; and as no late author that I am acquainted with, has entered into any detail on these subjects,

* *Cariosus Anglorum*. Gmel. Linn. Syst. Nat. p. 146.—*Tripoli*. Kirwan. El. Miner. p. 202.

† Vide Whitehurst's "Inquiry into the original state and formation of the Earth."

‡ Rotten stone also occurs at Wardlow Mire, and, as I am informed, at Ashford, and some other parts of the county; but I am not acquainted with the local circumstances with which it is attended in those places.

the

the following brief statement may not be unacceptable to those who are interested in geological inquiries.

1. The rotten stone, found on Bakewell Moor, is deposited on a lime-stone, which seemingly belongs to the *first* or *uppermost* stratum*.

Local situation.

2. It occurs in different parts of the moor; frequently on the surface of the lime-stone, immediately under the vegetable mould; but is procured in the greatest quantity in a long, or somewhat trough-shaped hollow, intersected by several broad irregular fissures, which are filled up with small fragments of lime-stone, the gravel-like *debris* (rubble) of the traversed stratum†.

3. In these fissures the rotten stone occurs at the depth of a few inches below the surface, and from that to ten or fifteen feet‡.

States.

4. It is procured in two distinct states. In one, the rotten stone, when dry, has an indurated, and sometimes even a stony consistence; texture; earthy; fracture, sometimes imperfectly conchoidal; at other times slaty; hardness, from that of chalk to that which does but just yield to the scraping of the knife (3—6. Kirwan); feels smooth, sometimes *rather* greasy—never so meagre as the foreign tripoli; does not crumble soon in water; effervesces *slightly* with acids; sp. gr. 2, 3. Its colour is usually between a brownish grey and isabella-yellow. The other variety occurs in a loose or pulverulent form; feels meagre; rarely effervesces with acids; sp. gr. 2, 2; its colour light yellowish grey.

5. The *hard* rotten stone (as the indurated kind is called by the rotten-stone *getters*) occurs in detached, nodular lumps, dispersed through the *rubble* above-noticed; the soft||, as a spongy earth or mud, either *coating* the more indurated variety; or deposited, in considerable quantities, under the *debris*, on the surface of the lime-stone rock.

6. Water, from the upper part of the moor, is constantly

* Vide Note A. † Vide Note B.
‡ Vide Note C. || Vide Note D.

draining

drained through the loose materials, which fill the hollows and fissures of the rotten-stone tract.

7. In this mineral depot are found, with the rotten-stone, fragments of chert; fragments of a calcareous stone in every possible state, intermediate between rotten-stone and perfect limestone; *rotten-stone with nuclei of solid black limestone*; *&c. &c.*

8. The calcareous stone, which forms, in these instances, the central parts of the nodular lumps of Rotten-stone, has the *external* characters of the black limestone or marble, found at Ashford-in-the-waters, &c. but differs, somewhat, in its *internal* properties, from any stratum of limestone yet discovered in Derbyshire.

9. Marine reliquia are sometimes found in the hard Rotten-stone; and these are generally such as have been observed to be most frequent in the black marble; viz. *Entomolithus Derbiensis Conchyliolithus Breynii*, &c. (v. Pet. Derb. T. 45, 39, &c)

Deduction that rotten-stone is produced from a limestone.

Such are the principal phenomena, which were noted during my examination of the depot of Rotten-stone near Bakewell.—The conclusions, to which this examination led, have been already alluded to; namely, *that Rotten-stone is produced by the disintegration of a particular variety of limestone, probably a black marble*; and that, consequently, authors are incorrect in considering the original substance of this fossil to have been an argillaceous stone.

It will here, however, be asked—how is the production of this particular substance from another, chemically as well as externally distinct, to be accounted for? and, if Rotten-stone be actually the result of a certain change in black marble or limestone, why is it not found in every situation, where such rock occurs? To answer these questions satisfactory will perhaps be impossible;—to answer them, however, in any way, without having recourse to the reciprocal transmutation of what have hitherto been considered, as simple, elementary parts

in mineral compositions*, we must first recur, it is evident, to the nature of the constituent matter of the original rock, as well as of the substance, which the disintegration of such rock has been presumed to produce.

Component parts of rotten-stone.

Limestones, it is well known, are composed principally of an indurated calcareous carbonate ;—Rotten-stone, according to the following analyses, of alumine in a loose or earthly form, and with its constituent particles in a very minute state of division—But we must remember, that many other principles enter into the composition of most limestones besides carbonate of lime; as alumine, silex, bitumen, and sometimes magnesia ;—and that Rotten-stone contains, besides alumine, silex, bitumen, or carbon, and frequently iron and calcareous earth ;—and that the comparative proportions of these component parts differ greatly in the different varieties both of limestone and rotten-stone.

Our analasis of rotten-stone has afforded the following results.

Analytical results.

1. *Very hard Rotten-stone, approaching Black Limestone in external appearance.*

| | |
|---|---|
| Alumine | 74 |
| Silex | 3 |
| Carbonate of Lime | 14 |
| Oxide of Iron | 2 |
| Inflammable matter and loss | 7 |
| | 100 |

2. *Another specimen of the hard variety, but of a light brown colour.*

| | |
|---|---|
| Alumine | 80 |
| Silex | 2 |
| Carbonate of Lime | 10 |
| Oxide of Iron | 1 |
| Inflammable matter and loss | 7 |
| | 100 |

* Transmutation of silex into lime, or that of lime into silex or alumine, however strongly contended for by some modern Geologists, most assuredly ought not to be assumed in any attempt to account for the phenomena of the mineral kingdom, till supported by stronger facts than those on which it rests at present.

3. *Hard*

3. *Hard Rotten-stone, but less indurated than specimen 2, colour nearly similar.*

| | |
|---|---|
| Alumine | 84 |
| Silex | 3 |
| Carbonate of Lime | 5 |
| Oxide of Iron | 0 |
| Inflammable matter and loss | 8 |
| | 100 |

4. *Soft Rotten-stone, i. e. with a texture much more loose or earthy than in the other specimens.*

| | |
|---|---|
| Alumine | 87 |
| Silex | 4 |
| Carbonate of Lime | 0 |
| Oxide of Iron | 0 |
| Inflammable matter and loss | 9* |
| | 100 |

If we compare the foregoing analysis with those, which mineralogists have given us of limestones, we shall find, that the chief difference (in a chemical point of view) between rotten stone and certain varieties of limestone, exists in the

* It should be observed that the "*loss*," in these analysis, never exceeded 1·5; hence the proportion of "*inflammable matter*" may be stated as varying from 5·5 to 7·5. At the time of making my experiments on rotten-stone, the principal object in view was to ascertain the predominating earth in its composition, and not determining the nature of the inflammable matter, it was placed with the loss;—there can be little doubt, however, of its being *carbon*. Silex was found in all the specimens examined. Carbonate of lime only in the harder varieties, and not constantly in those. Two or three specimens analysed, in all external respects similar to No. 3, were without it. Oxide of Iron was only present in the harder rotten-stones.—The actual constituents, therefore, of genuine or perfect rotten-stone (that is, rotten-stone in which the disintegration of the original substance is complete) may be stated to be *alumine, silex,* and *inflammable matter (carbon?)*.

 larger

proportion of alumine, which the former of these substances contains, and its comparative, or, in some instances, its total want of the carbonate of lime. The particular varieties of limestone now alluded to, are those which Mr. Kirwan has denominated *argilliferous marlites*, on account of their holding a large proportion of *argill (alumine)* in their compositions. (v. E. Min. v. 1. p. 99.)—Some of these stones, though affording lime, contain 30 per ct. of alumine, together with small quantities of silex, iron, &c. :—and our Derbyshire black marble, or limestone, undoubtedly belongs to this class.—

**Black marble at Ashford-in-the-waters.** The greatest quantity of this stone is quarried at Ashford-in-the-waters ; and, as the quarry is situated at no great distance from the *depot* of Rotten-stone, and affords an excellent example of this formation, I shall here describe the state in which it is found, and some of its principal varieties. It occurs in beds, which vary in thickness, from a few inches to two or three feet, with interposed seams *(semistrata)* of black, bituminous shale and clay. The substance of these beds, though throughout of the same general aspect, and constantly *burning to lime*, more or less pure, differs greatly in the proportion of its constituent parts, as well as somewhat in its external characters. The limestone of those beds, immediately worked as marble, is of a deep greyish black, which, on the stones being polished, becomes perfect, or dark-black :*—texture close, fine earthy :

* Its colour must be ascribed to the bitumen or carbon, which it contains, as it becomes perfectly white, when calcined, and also acquires a white, or as.-coloured, crust, on exposure to the weather. In many instances I have found the crust of a considerable thickness, and become perfect Rotten-stone. And there is no doubt but in walls, which are sometimes built of black marble, and in other exposed situations, this would frequently be the case, if a further decay of the stones were not prevented by a timely and friendly covering of lichens and mosses. I have observed, however, that pieces of polished marble, though equally exposed with those in the unpolished state, do not so soon acquire a white crust.—Polishing, by filling up the minute interstices, induces a greater degree of external hardness of the stone, and prevents for a longer time the decomposition of the surface.

fracture

fracture slaty, passing into the imperfectly conchoidal* ; hardness from six to seven (Kirwan. p. 38) : emits a fetid or rather urinous smell when scraped, but in a much less degree than the following varieties: contains, according to the specimen examined, about 18 per ct. of alumine, with small proportions† of silex, iron, and inflammable matter.

Other varieties.

The next variety of limestone it will be proper to notice, is one rejected by the workmen at Ashford, as being less fit for their purpose than that I have just described.—It appears to be too soft to receive a lasting polish, and its colour, though black, is much less deep than in the foregoing variety, frequently verging on brownish black: texture earthy : fracture slaty : hardness 6 : gives out a very fetid smell on being scraped. One specimen of this stone contained, according to the experiments made on it, 66 carbonate of lime ; 24 alumine ; 1·2 oxide of iron ; 1·5 silex ; and 7 inflammable matter. Another specimen of this stone, however, *from the same bed,* yielded only 19 alumine.

A third strongly marked variety of limestone, found with the foregoing, has the following characters: colour black, or brownish-black : texture splintery, with disseminated, shining, spar-like particles ; these frequently exhibit the minute parts of organic remains : fracture slaty : hardness 7 : emits a very fetid odour, when scraped or rubbed. The specimen analysed gave 8 per ct. alumine, and 4 silex, with 7 inflammable matter, but little or no trace of iron.

It must now be observed, that along with these three descri-

* By *fracture* is here meant the *general appearance,* or form, which the broken surface of the fossil presents : by *texture,* the grain, or form and disposition of the particles, observable throughout the surface of the *fracture.*

† In no instance did the proportion of silex exceed 4 per ct. or that of the iron 1½. As the experiments, however, which gave these results, were not repeated on each variety of stone, we do not give these proportions as those which analysis hereafter may find to be correct.—The proportion of alumine, in each instance, we believe, will be found to be near the truth.

bed

Other varieties.

bed varieties of limestone, several others occur, which, in their external characters, exhibit various gradations between the black marble and the bituminous shale, that separates the calcareous beds; and that the whole formation of these limestone *stratula* appears to graduate, or to pass, by an almost insensible transition, into the great *stratum* of shale, under which the limestone of Derbyshire, for the most part, dips.

It is evident, from the above remarks on the black limestone formation, that among its numerous beds the orginal of *Rotten-stone* probably exists; and, though the result of my own experiments and observations certainly does not warrant the conclusion, that it has yet been detected as a native rock or stratum, there seems little doubt, but that a more careful examination, than what my leisure when at Ashford permitted *me* to make, may hereafter determine the stone in this state. The variety of black limestone already described, as holding, sometimes, 24 per ct. of alumine, undoubtedly comes near in external characters to the central nodules of marble, which, it has been observed, occur frequently as *nuclei* to the fragments of *hard* Rotten stone, (v. p. 317) and which, there is every reason to conclude, are remaining portions of the original calcareous rock. Still, however, this rock appears to have differed essentially from the limestone, with which we are now comparing it: 1st. in being a somewhat softer stone; 2d. in containing a much larger proportion of inflammable matter; and, lastly, in holding, at least, 30 per ct. of alumine.* It may here, perhaps, be objected, that a stone, holding even 30 per ct. of alumine, can never be presumed to give by its decomposition, a substance containing more than double such proportion of the material—especially as this substance is evidently *not* composed (in certain instances at least) of the *travelled*, and at length deposited, particles of the original stone; but actually exhibits the matter (in part) of the original stone itself under its primitive structure, and merely deprived of one of the constituent principles.

* All the specimens I have examined, have given something more than the proportion of alumine here stated.

For

For this really seems to be the state, in which the greater part of the indurated rotten stone occurs. To this objection, I can only, at present, oppose, as probable, the supposition, that, during the formation of *hard* rotten stone, while loosing the calcareous particles, a gradual and considerable contraction took place in the remaining matter; and that this was effected without destroying the slaty structure, where it previously existed, in the primary stone.* By this assumed contraction in the substance of rotten stone, it is evident, we may readily account for the greater proportion of alumine it exhibits, on comparing a given quantity of it with an equal one of limestone. But it will, probably, be advanced, that the hypothesis eventually supports more than we wish to prove; as, admitting the contraction of the matter forming rotten-stone, any limestone holding a small quantity of alumine, may be the original stone. The local circumstances, however, attendant on rotten stone, must prevent such a supposition from being adopted. All limestones, it is true, are liable to decomposition; and the black *seem* to be more subject to this process† than the lighter.

* A nearer approximation of the aluminous particles to each other, may easily be supposed as a natural consequence of the removal of the calcareous matter; but that the structure of the original stone should remain, after this loss of matter, will not, perhaps, be as easily supposed or admitted. However, as the ingredients of black limestones, &c. exist (it is probable) merely in the state of *mixture*, the extraction of any one of these constituent parts will certainly be less liable to destroy the general structure of the stone, than if the process had to act on principles *chemically* united.

We have here considered the structure, or fracture of hard rotten stone to be immediately derived, generally speaking, from that of the original limestone; but in some instances, particularly where the slaty structure is present, it is rather, perhaps, the consequence of the *contraction* contended for, than the remains of any particular disposition of particles, which existed in the primary fossil. We have, not unfrequently, observed the slaty structure in hard rotten stone, where no vestige of it appeared in the enclosed *nuclei* of limestone; though, it must be observed, that these *nuclei*, in every other respect, were perfectly similar to those in which such structure was very evident.

† Vide Note.

*** The Editors regret that this paper was left in an unfinished state, owing to the death of the ingenious author, and that several of the notes referred to, have not been discovered amongst his manucripts, though these have been examined with very great care and attention.

---

VIII.

*On the Measure of Moving Force. By Mr.* PETER EWART*.

Controversy, longago, about the measure of force.

IN the theory of mechanics, forces are understood to be mathematical quantities, capable of being measured and compared with as much certainty as lines, or surfaces, or any other mathematical quantities. Respecting the principles, however, of this measurement and comparison, various doctrines have been held. A controversy on this subject, after having been long and warmly agitated by learned men in different parts of Europe, appears, about seventy years ago, to have gradually subsided ;† and since that period, it has been the prevailing opinion with mathematicians, that the argument respecting the measure of the force of a body in motion, was merely a dispute about terms, and that, though the force in question may be variously estimated, according to circumstances, it is most naturally and consistently expressed by the product arising from the mass being multiplied into its velocity.

The rule that the effect is as the mass multiplied by sq. veloc. is of great practical value.

Although scientific men have, for more than half a century, been generally satisfied on this question, it must nevertheless be acknowledged, that considerable difficulties have occurred in the practical application of their measure of force ; and, it is remarkable, that the measure which they have rejected, appears to

* Manchester Mem. viii. (or II. N. S.)

† Dr. Reid says, "it was dropt rather than ended, to the no small discredit of mathematics, which hath always boasted of a degree of evidence inconsistent with debates that can be brought to no issue." Essay on Quantity.—Philosophical Transactions, 1748.

to have been first suggested to Hook and Huygens, by their practical observations on the motion of pendulums, and was afterwards adopted by Smeaton, as a rule for the great operations in which he had so much experience.

It is much to be regretted, that theory should appear to be at variance with practice, or that any ambiguity should remain on a question of such general application in mechanics.

Opinions influence practice.

It has often been asserted, indeed, that practical operations need not be affected by differences of opinion about the measure of force; for, there being no disputed facts, the mere scientific explanation of the phenomena, it is said, can be of little importance to practical men.

—— as is well stated by Smeaton.

On this point, however, Mr. Smeaton's observations merit particular attention. He says, in reference to mistaken notions about the measure of force, "that not only himself and other practical artists, but also some of the most approved writers, had been liable to fall into errors, in applying the doctrines of force to practical mechanics, by sometimes forgetting or neglecting the due regard which ought to be had to collateral circumstances. Some of these errors are not only very considerable in themselves, but also of great consequence to the public, as they tend greatly to mislead the practical artist in works that occur daily, and which require very great sums in their execution*."

Notwithstanding Mr. Smeaton's excellent experiments and observations on this subject, exhibiting much want of agreement between the theory usually given, and the practical results, the mechanical principles of force continue to be treated nearly as before; and, I believe, we are not without recent instances of errors similar to those which he has noticed.

Atwood's Statement.

Mr. Atwood, in his Treatise on the rectilinear motion and rotation of Bodies, bestowed considerable attention on Mr. Smeaton's experiments and conclusions. He also observes, that Emerson, and other authors of merit, have been led into considerable errors, "by supposing the momentum of bodies

* Philosophical Transactions, vol. 66, part 2d. p. 452.

to

to be as the quantity of matter into the velocity*. In that he agrees with Mr. Smeaton; but he afterwards concludes, that neither of the measures of force are capable of general application, and that for *one* class of the effects of force, we have no proper measure.

—— that a permanent enumeration of force cannot be had by either of the measures.

After discussing various examples of force, he proceeds as follows: "But the truth is, the principle (of permanent quantity) obtains not according to either of the measures, except in particular cases, which may be demonstrated as the other properties of forces are from the general laws or axioms.

Instances.

"In the rectilinear motion of bodies, accelerated from quiescence, or retarded until they are at rest, the permanency of any given quantity of motion is demonstrated from the axioms, whether that motion be estimated by one measure or the other.

"In bodies which revolve round fixed axes, the principle obtains, without exception, when the momentum is measured by the quantity of matter into the square of the velocity, but fails when measured by the quantity of matter into the velocity; a given quantity of motion thus estimated being alterable in any assigned ratio.

"In the communication of motion to bodies by collision, when the direction of the stroke passes through the centre of gravity, the principle in question holds universally, according to the measure of the mass into its velocity, but fails when the momenta are estimated by the mass into the square of the velocity in every case, except when both bodies are perfectly elastic, or one perfectly elastic, and the other perfectly hard.

"Lastly, when motion is communicated to bodies by impact, the direction of which passes not through the centres of gravity, the quantity of motion communicated, whether estimated by one measure or the other, preserves neither equality, nor any constant proportion to the quantity of motion impressed†."

These conclusions appear to be rather paradoxical, but they are neither new nor uncommon.

* Treatise on Rectilinear and Rotatory Motion. Preface, p. 10.

† Ibid, p. 366—368.

If

It is true they have not been usually stated in the same terms: but I believe the same inferences strictly follow from the reasoning of many other good writers on this subject. If forces be mathematical quantities, we may reasonably enquire, how is it that they are so indeterminate in relative magnitude?

Question. Why the relation of the magnitudes of forces should be difficult to express.

If two given lines, angles, surfaces, or solids, be equal, they are equal in whatever manner they may be applied, or however they may be measured. But if we have two given bodies, moving with velocities inversely as their masses, their forces, it would appear, are either equal or unequal, according as they may be classed under one or other of the above subdivisions of mechanical phenomena.

If the forces of two given bodies in motion are either equal or unequal, according to the purpose to which they may be applied, it would be very desirable to have a complete and accurate classification of all the phenomena of force, exhibiting the variations to which they may be subject; and we are so far indebted to Mr. Atwood, that he is, I believe, the only author who has attempted to make such an arrangement. But his arrangement is not complete, for he has omitted to include in it many important practical applications of force; such, for example, as the raising of a body to a given height, where it is to be left at rest; the driving of piles;—the overcoming of friction;—the grinding of corn;—the hammering and rolling of metals; and various other applications of force of a similar kind.

Classification of the phenomena desirable.

Mr. Atwood appears, however, to have been aware that the doctrines of force, as they are usually treated, could not be of much service in practice; for a little farther on he observes, "It is not probable, that the theory of motion, however incontestible its principles may be, can afford much assistance to the practical mechanic; and there appears as little room to imagine, that any errors or misconceptions which may have been propagated concerning the effects of forces considered in a theoretical view, have at all impeded the due construction of useful machines, such as are impelled by the force of wind or water,

The doctrines of force as usually treated are of little practical value.

Whence it is supposed that errors in theory have not been noxious.

water, by springs or any other kind of motive power. Machines of this sort, owe their origin and improvement to other sources: it is from long experience of repeated trials, errors, deliberations, corrections, continued through the lives of individuals, and by successive generations of them, that sciences, strictly called practical, derive their gradual advancement from feeble and awkward beginnings, to their most perfect state of excellence*."

Smeaton has shewn the contrary.

But he has, in this instance, I apprehend, pressed his argument rather too far; and he is quite at variance with Mr. Smeaton, who has pointed out many inconsistencies in theoretical conclusions, which have been carried into practice with most injurious effects†.

Advantages of theory to practical men.

It cannot be doubted, that ingenious men, of rare natural endowments, have, without any scientific aid, accomplished wonders in the invention and improvement of machinery. But how can it be supposed that these men could have derived no assistance from a clear and sound knowledge of the principles of mechanics? Every new combination presented to their minds must have involved them in new, and repeated labours to ascertain its effects; and these labours must have frequently terminated in a conviction, that their time and pains had been

* Treatise on Rectil. and Rotat. Motion, p. 381.

† See Philosophical Transactions, vol. 66, part 2d. p. 452, &c. and the following note, p. 454. "Belidore (Arch. Hydr.) greatly prefers the application of water to an undershot mill, instead of overshot; and attempts to demonstrate, that water, applied undershot; will do six times more execution than the same applied overshot. See vol. i. p. 286. While Desaguliers, endeavouring to invalidate what had been advanced by Belidore, and greatly preferring an overshot to an undershot, says, (Annotations on Lecture 12. vol. 2 p. 532.) that from his own experience, "a well made overshot mill, ground as much corn in the same time, with ten times less water;" so that betwixt Belidore and Desaguliers, here is a difference of no less than 60 to 1.---*Smeaton*.

Each of these authors has been considered by many as the best authority for practical men; and their various inconsistent rules have often been adopted, in the construction of expensive machines, in this country, as well as on the continent.

wasted

wasted in examining old facts under new appearances. Such disappointments have sometimes served indeed rather to stimulate than to damp their zeal for making farther discoveries. But if a good theory in physical science be understood to comprehend a distinct arrangement of what is known on the subject; or if it furnish the means of applying the experience of one case so as to determine the result of another of the same kind, but different in degree, or under different circumstances; it cannot be questioned that such information must tend to shorten the labours, and smooth the path of the ingenious inventor; and still more valuable must it be to those whose task it is to distinguish the curious from the useful, and to carry into execution the real but not the fanciful improvements.

Great inventors who have excelled in theory.

Neither does it appear that Mr. Atwood is supported in his opinion, by the history of useful discoveries in mechanics. If Huygens and Hooke had not been scientific as well as ingenious men, we might possibly have been still ignorant of the properties of the balance regulated by springs. If Smeaton had not availed himself of just theory, as well as experiment, we might still have had to learn the principles by which we must be guided in applying water to the best advantage as a moving power. If a clear and strong understanding, and a mind richly stored with scientific attainments, had not been combined with wonderful fertility of invention, in the justly celebrated improver of the steam-engine; incalculable labour might still have been wasted in performing operations which are now accomplishhd with as much ease and regularity as the gentle motions of a time-piece.

Theory is also advantageous.

But if it were even granted, that all these distinguished men might have attained their object without the aid of theory; it must still be acknowledged, that to those who have to follow their steps, and to apply their inventions and improvements to various purposes, under various circumstances, it must be of essential importance to be free from perplexity in the principles by which they must be governed; and it is under this impression that I have been induced to state to this society some of the difficulties which have occurred to myself, in common, I believe, with

with many other practical men, in the application of the prevailing doctrines of moving forces; in the hopes that others, better qualified for the task, may be prevailed upon to reconsider the subject; and remove the obscurities in which some parts of it appear to be involved.

Cases of difficulty in the doctrines of moving force.

I shall first briefly describe some particular cases where these difficulties occur, divesting them as much as possible of all complicated circumstances; and I shall be careful to state such facts only as will be readily admitted by any one moderately acquainted with the subject. I will then quote, from approved writers on mechanics, such observations as appear to have been given in explanation of the points in question, accompanied with some remarks which they seem to require; and I shall conclude, by venturing to offer some farther explanations, which appear to me to be capable of general application in mechanics.

*Examples of Force producing Motion in Bodies from a State of Rest.*

1. If two balls, A and B, (figure 1.) whose masses are as 1 to 4, be suspended like pendulums; and if they be set in motion by two equal weights, C and D, acting on them by means of the bent levers, E and F, whose fulcra are fixed, and whose perpendicular arms are equal, but the length of the horizontal arm of F, twice the length of the corresponding arm of E. If C descends through the space S, D will descend through an equal space in the same time; and by these equal forces in equal times, A will have acquired exactly twice the velocity of B. Now if these effects are to be measured by the products of the masses into their velocities, D produces twice the effect of C, although their forces are precisely equal.

In this and the following cases, the mass of the lever, &c. is supposed to be indefinitely small, when compared with that of the ball which it moves.

2. If we suppose two balls, *m* and *n*, (fig. 2.) whose masses are as 1 to 2, to be suspended as in the last case, and put in motion

motion by the pressure of the atmosphere on the pistons P and Q acting upon $m$ and $n$, by means of the levers G I and A B; A F being equal to B F, but G H=2 H I, and the area of the cylinder E twice that of C; supposing these cylinders and the fulcra F and H to be immovable, and the space under each piston to be a vacuum. Then P and Q will move through equal spaces in equal times, and $m$ will acquire just twice the velocity of $n$.

Cases of difficulty in the doctrine of moving force.

Here the force of P is twice that of Q, but the effects of these forces, if estimated by the product of each mass into its velocity, are equal.

3. In treating of rotatory motion;—in finding, for example, the centre of gyration of a mass revolving about a fixed point, the *rotatory force* of each particle is universally understood to be as the square of its distance from that point, or as the square of its velocity. If a body, A, (fig. 3) be made to revolve about the centre C, by a force acting at P; four times that force, applied at the same point, P, will be required to make a body, B, equal to A, placed at twice the distance of A from C, revolve with the same angular velocity, that is, with twice the absolute velocity of A. If both the bodies be disengaged from C, they will each continue to move with the same velocity as before, but in rectilinear directions; and then the force of B is said to be only twice that of A. But it is not alleged that A can gain, or B lose force, by the mere circumstance of being disengaged from C. How then is this change in their relative forces to be accounted for?

4. Let the lengths of the arms A F, F B, (fig. 4.) of the balance beam, A B, be in the proportion of 1 to 2, and let the weight of the ball, $m$, be to that of $n$, as 2 to one. If they vibrate about the fixed fulcrum F, the quantity of motion of $m$, will be equal to the quantity of the motion of $n$. Let C D be another balance beam, and let C G and G D be each equal to A F, and the weights of $o$ and $p$ be each equal to that of $m$, and let A and C move with equal velocities. If the quantity of motion of $m$ be equal to that of $n$, the quantity of motion of $p$ must also be equal to that of $n$; and the sum of the quantities of

motion

Cases of difficulty in the doctrines of moving force.

motion of $o$ and $p$ must be equal to the sum of the quantities of motion of $m$ and $n$. But let both beams be at rest, and let the pressure of 2 be applied for a given time to C, to generate velocity in $o$ and $p$; a pressure of 3 will be required to be applied to A for an equal time, and through an equal space, to generate an equal velocity in $m$. The generating forces, therefore, are as 2 to 3, although the quantities of motion generated by these forces are equal.

5. Let G (fig. 5.) be the centre of gravity of two bodies, A and B, connected by an elastic rod, at rest, but free to move in any direction; and let a given quantity of motion be communicated at any point, D, in a direction at right angles to the rod, Mr. Vince has demonstrated that the velocity of G will be the same wherever the motion is communicated*; that is, if a given *force* be applied, or *quantity* of *motion* communicated at G, a progressive motion of the mass, without any rotatory motion, will be the result; but if the same force be applied at any other point D, we shall have the same progressive motion, and a rotatory motion besides.

Is that rotatory motion produced without force?

### *Examples of Motion destroyed, and of Motion transferred from one body to another.*

6. If the weight of the ball, A, (fig. 6.) be to that of B, as 2 to 1, and if they move in opposite directions with velocities reciprocally as their weights, and strike at the same instant the ends of the spring, S. If the strength of the spring be such, that the balls shall be at rest when its ends are brought to meet; they will meet at E, DE being equal to 2 CE. Here the effect produced is the compression of the spring. But though the quantity of motion of A is equal to that of B, the portion of the effect produced by A, is less than that which is produced by B.

If we substitute for B a ball equal in weight and velocity to

* Philosophical Transactions, vol. 70, p. 551.

A,

A, the ends of the spring will not be brought to meet by the action of the balls. In that case, when the balls are at rest, the distance between the ends of the spring will be to CD, as 1.1 to 6 nearly.

Cases of difficulty in the doctrines of moving force.

7. If a non-elastic mass, A, (fig. 7.) moving with a given velocity, strike an equal non-elastic mass, B, at rest in free space; both balls will move on together, with half the velocity of A. Upon the principle of the moving forces being as the quantities of motion, and the quantities of motion as the masses into their velocities; it is held that the moving force of A is equal to that of A and B, moving together with half the original velocity of A.

If the ball B, have a spring attached to it, furnished with a toothed catch C, to retain the spring in the form to which it may be compressed; it will then represent a perfectly non-elastic body. Let A strike the spring and compress it to E, and let A and B move on together, with half the original velocity of A. Let the spring be then removed in its compressed state, and placed between to other balls, C and D, equal in their masses to A and B, and at rest in free space; let the catch C, be then disengaged; the spring will resume its original shape, and the balls, C and D, will each move off with half the original velocity of A; and we shall then have three masses besides A, each equal to A, moving with half the original velocity of A, and all of them deriving their motion from the original force of A.

8. Let A (fig. 8.) be a non-elastic soft mass, uniformly penetrable by the cylinder $c$; that is, the tenacity of the parts of A shall be such, that $c$ shall meet with the same resistance at every point of its progress. Let A move with the velocity $v$, in the direction A B, against an immovable obstacle, and be brought to rest by forcing the length E F of the cylinder into the ball. That penetration of $c$ is, in this instance, the whole effect produced by the force of the motion of A. Let the operation be repeated, but instead of an immoveable obstacle, let B be a mass equal to A, in free space, but not penetrable by $c$: then the cylinder will be forced into A a depth equal only to $\frac{1}{2}$ E F,

 and

Cases of difficulty in the doctrines of moving force.

and if F H and F I be taken each $= \frac{1}{4}$ E F, when the side of A has arrived opposite to H, the side of B will have arrived opposite to I, (as represented at No. 2.) and the velocity of both balls will be $\frac{1}{2}\, v$.

If we repeat the experiment with a ball of half the weight, and twice the velocity of A, striking B in free space, the effects will be very different. We must then have a longer cylinder; for the length of it forced into the ball will be $= \frac{4}{3}$ E F, and the velocity of both balls after collision will be $\frac{2}{3}\, v$. It is not easy to understand how these last effects can be produced by a force no greater than the first.

9. It is argued that the mass into the velocity must be the proper measure of the force of a body in motion, because the sum of the products of the various masses of any system of bodies into their respective velocities, is always the same in the same direction, unless acted upon by some external force. In other words, because the motion of the centre of gravity of any system of bodies cannot be changed or disturbed by any action of those bodies upon each other.

If two equal non-elastic balls A and B, whose common centre of gravity is G, (fig. 9.) move with the velocities and in the directions A C and B C, oblique to each other, they will meet at C, and after collision they will move on together with the velocity and in the direction G C. If the product of the mass into the velocity *in the same direction* be taken as the measure of the moving force, we have in the *motion* of these bodies equal effects of force before and after collision. But it is obvious, that to produce the separate motions of A and B before collision, much greater force must be required than to produce the motion of their joint mass.

*(To be continued.)*

## IX.

*Classification of certain Luminous Appearances which result from the Reflection or Refraction of Light by Clouds, and which are commonly called Halos, Rainbows, Parhelia, &c. By Mr.* THOMAS FORSTER, *F. L. S**.

EVERY one who is conversant in meteorology, must be well acquainted with such luminous appearances, occasionally seen about the sun, moon, and planets, and caused by the refraction of their light through a cloud of peculiar structure, as are usually called halos, coronæ, burrs, glories, &c. But these phænomena have hitherto received no definite names whereby they may be distinguished from each other, though they differ considerably in appearance. Meteorologists have spoken of halos and coronæ indiscriminately, without distinguishing between the corona or luminous disk, and the halo, or luminous ring. **Luminous appearances in clouds or vapour.**

The ancient writers, too, spoke differently of halones, circuli, coronæ, halyses, parhelia, and other the like phænomena, as appears by the works of Aristotle,† Pliny,‡ Seneca § and others. Aristotle appears to have written with the most perspicuity of all of them.

With a view to obviate the inconvenience and misunderstanding which might arise from the confusion or promiscuous use of terms not sufficiently definite, I subjoin the following classification, which, though imperfect, may serve, till a better shall be found, to enable meteorologists, in their journals, to express, with tolerable precision, the kind of appearance which they wish to commemorate.

I endeavour to classify them (for want of a better criterion) according to the various *shapes* or *figures* which they present. **Classification according to their figure.**

* From his "Researches into Atmospheric Phenomena."

† Aristot. Meteor. lib. iii. cc. 2, 3.

‡ Plin. Hist. Nat. lib. ii. cc. 29, 30, 31, 32. lib. xviii. 35.

§ Senec. Opera Philos. lib. i. cc. 2, 3, 4, 5, 6, 7.

It

It must be remembered, that their various figures are the result of the particular construction of the cloud which refracts their light: a correct attention, therefore, to these appearances, may lead to a more perfect knowledge of the structure of the refracting medium.

Halo.

HALO.* Def. *Circulus vel Annulus lucidus aream includens, in cujus centro Sol aut Luna apparet.*

*Obser.* By a halo I understand an extensive luminous ring, including a circular area, in the centre of which the sun or moon appears; whose light, passing through the intervening cloud, gives rise to the phenomenon. Halones are called *Lunar* or *Solar,* according as they appear round the moon or sun. Those about the moon are the most common. They are generally pretty correct circles: I once, however, saw a halo of a somewhat oval figure. Halones are sometimes coloured with the tints of the rainbow†.

Double halo.

HALO DUPLEX. *Duo Annuli, in quorum centro communi Sol aut Luna videatur.*

*Obser.* A double halo is not a very common occurrence. I have observed, that simple halones are generally about 45° in diameter: in case of double halo, it might be worth while to take the diameters of each of the concentric circles.

Triple halo.

HALO TRIPLEX. *Tres Annuli, in quorum centro communi Sol aut Luna apparent.*

*Obser.* Triple halones are extremely rare occurrences.

Discoid halo.

HALO DISCOIDES. *Annulus aream reliquâ nubis parte lucidiorem continens, in cujus centro Luna aut Sol visus est.*

*Obser.* A discoid halo may be said to be a halo constituting

* The word *halo*, or *halos*, is evidently derived from the Greek ἅλων or ἅλως, signifying an *area.* The Latin writers appear to have spoken indifferently of halones, halyses, coronæ, circuli, &c. without sufficiently distinguishing between the *corona* and the *halo*—in other words, between the *luminous disk* and the *luminous ring.*

† The coloured halo is generally seen in a denser cloud.

the

the boundary of a large corona: it is generally of less diameter than usual, and often coloured with the tints of the *Iris*. A beautiful one appeared on the 22d of December, 1809, about midnight, during the passage of a *cirrostratus* before the moon.

CORONA. *Discus lucidus, vel portio circularis nubis reliquâ lucidior, in cujus centro Sol aut Luna videtur.* Corona.

*Obser.* When the sun or moon is seen through a thin cloud, a portion of the cloud, more immediately round the sun or moon, appears much lighter than the rest of it: this luminous disk, if I may be allowed the expression, I call a *corona*.

Coronæ are of various sizes, according to the peculiarities of the intervening vapour: but they seldom exceed 10° in diameter: they are generally faintly coloured at their edges.

Frequently, when there is a halo encircling the moon, there is a small *corona* more immediately round it. Coronæ, as well as halones, have been always observed to prognosticate rain, hail, or snow. As far as I can observe, they are generally seen in the *cirrostratus* cloud.

CORONA DUPLEX. *Discus lucidus, alium discum paulo lucidiorem ac minorem includens, in quorum centro communi Sol vel Luna videta.* Double corona.

*Obser.* A double corona is very common: sometimes they are triple or quadruple.

PARHELION. Def. *Imago Solis falsa, vel plures imagines eiusdem generis circa Solem circulatim dispositæ, et magis minúsve halonibus aliisque lucidis vittis comitatæ.* Parhelion.

*Obser.* Parhelia vary considerably in general appearance: sometimes the sun is encircled by a large halo, in the circumference of which the mock suns usually appear: these have often small halones round them: they have usually a horizontal band of white light, of a pyramidal figure, extending from them: sometimes a large semicircular band of light, like an inverted arch, seems to rest upon the halo which encircles the sun: but these

these phænomena vary too much to be particularly described here: their peculiarities ought to be minutely observed, and noted down in a Meteorological Journal.

Paraselene.

PARASELENE. *Lunæ imago falsa, vel plures imagines huius generis circa Lunam dispositæ, et magis minúsve Halonibus, aliisque lucidis vittis comitatæ.*

*Obser.* The *paraselene*, the *parhelion*, and the several kinds of *halo* and *corona*, all appear to result from the intervention of cloud between the spectator and the sun or moon, through which the light passes; but there is another well-known phenomenon, which always appears in a cloud opposite to the sun or moon; namely, the

Iris or Rainbow.

IRIS. Def. *Circulus maximus coloratus in Nube Soli oppositâ visus, et cuius centrum centro Solis oppositum est, qui, quòd portio eius tantùm videtur, arcus adparet.*

*Obser.* The rainbow is an appearance too familiar to every one to need any particular description. As the halo and corona appear generally in the *cirrostratus* cloud; so the *Iris* appears always in the *nimbus*. Lunar rainbows are very rare occurrences.

Double Rainbow.

IRIS DUPLEX. Def. *Duo Circuli colororati quorum centrum commune Solis centro opponitur qui quòd eorum portiones tantùm videantur Arcus adpareant.*

Double rainbows are not unfrequent. The order of colours in the outer one is reversed,* They are mentioned by Aratus.†

Colourless Rainbow.

IRIS VNICOLOR. *Circulus maximus colorum exsors, in nube visus, et cujus centrum centro Solis vel Lunae opponitur; qui quod portio ejus tantum videatur Arcus adpareat.*

The Iris unicolor is more properly a colourless rainbow, and

* Arist. Meteor. lib. iii. cap. 5.
† Arat. 208.

appears

appears in the mist. Such a one appeared on 11th November, 1811, near Newington, in Middlesex.*

Rabdi divergentes. *Radii Solis radiantes ob quandam specialis generis interpositam nubem.* Diverging beams.

The remarkable appearance of the sun's rays, in a cloud before rain, has been alluded to by Aristotle,† Virgil,‡ and others.

Rabdus pyramidalis. *Portio pyramidalis lucis in nube visa, quasi ex Sole procedens, cujus vertex diametro Solis horizontali perpendicularis est.* Pyramidal beams.

Not uncommon in haze of a peculiar kind, (perhaps cirrostratus.) Sometimes, small portions of the rainbow appear. I observed this between seven and eight o'clock, 21st August, 1811, in Sussex, near Wadhurst.

*(Discussions upon these hereafter.)*

---

## SCIENTIFIC NEWS.

### *Naturalist's Miscellany.*

The Nobility, Gentry, and the Public in general, are most respectfully informed, that a New Series of the Naturalist's Miscellany will be continued as soon as arrangements for that purpose can be completed, and of which due notice will be given. General Indexes, in Latin and English, of the subjects contained in the first twenty-four volumes, will be published September 15th, 1813, by Elizabeth Nodder and Son, 34, Tavistock-street, Covent Garden, where complete sets or single numbers of that work may be had; as also of their *Flora Rustica.*

* Annals of Philosoph. by Dr. Thompson, p. 80.
† Aristotle Meteor. lib. iii. c. 2.
‡ Virgil Georg. lib. i. l. 445.

*Medical*

*Medical Schools of St. Thomas's and Guy's Hospitals.*

The winter Course of Lectures at these adjoining hospitals will commence the beginning of October, viz.

*At St. Thomas's*

Anatomy and the Operations of Surgery, by Mr. Astley Cooper, and Mr. Henry Cline. Principles and Practice of Surgery, by Mr. Astley Cooper.

*At Guy's*

Practice of Medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine, and Materia-Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and diseases of women and children, by Dr. Haighton. Physiology, or Laws of the Animal Economy, by Dr. Haighton.—Structure and Diseases of the Teeth, by Mr. Fox.

N. B.—These several lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a Complete Course of Medical and Chirurgical Instruction. Terms and other particulars may be learnt at the respective hospitals.

---

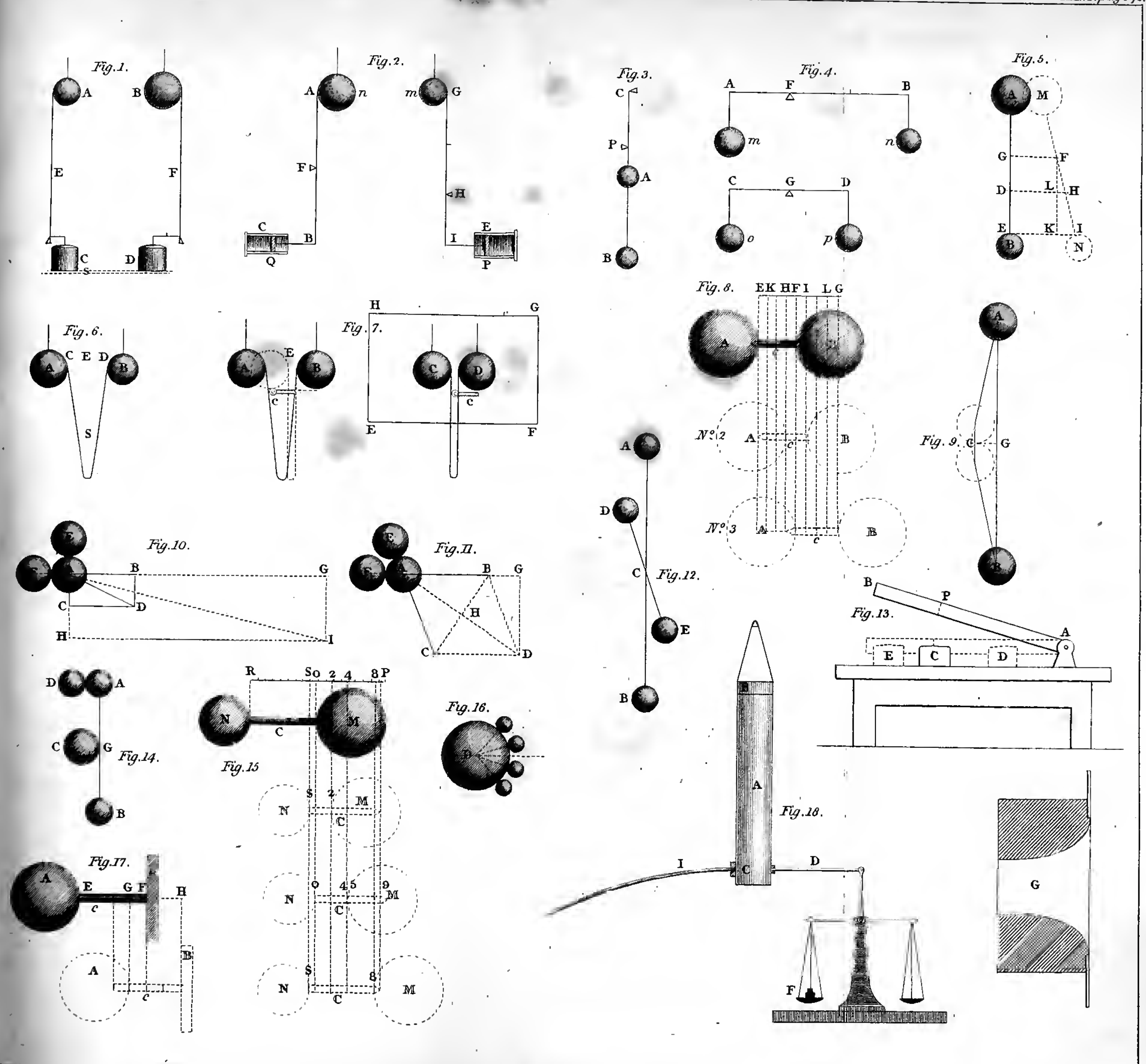
*Dr. Clarke and Mr. Clarke's Lectures on Midwifery, and the Diseases of Women and Children.*

Dr. Clarke and Mr. Clarke will begin their winter Course of Lectures on the Diseases of Women and Children, on Monday, October 4th. The lectures will be read at the house of Mr. Clarke, No. 10, Upper John Street, Golden Square, from a quarter past ten till a quarter past eleven, for the convenience of students attending the hospitals.

For particulars apply to Dr. Clarke, New Burlington-street, or to Mr. Clarke, No. 10, Upper John Street, Golden Square.

---

Mr. Hodgson's Treatise on the Diseases of Arteries and Veins, comprising the Pathology and Treatment of Aneurisms and wounded Arteries, will be published in October, in one volume 8vo. illustrated by a series of engravings in 4to.




Thomson & Hall sculp. 14 Bury Str. Bloomsb.

A

# JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

*OCTOBER*, 1813.

## ARTICLE I.

*Additional Observations on the Effects of Magnesia in preventing an increased Formation of uric Acid; with Remarks on the Influence of Acids upon the Composition of the Urine. By* WILLIAM THOMAS BRANDE, *Esq. F. R. S. Prof. Chem. R. I**.

IN a paper which I had the honour of laying before this Society, about three years ago, and which is published in the Philosophical Transactions†, some cases are related, illustrating the effects of magnesia in preventing an increased formation of uric acid, and some experiments are detailed, instituted with a view to discover its mode of action. **Reference to former paper.**

Since that period, many opportunities have occurred, both to Sir Everard Home and myself, of confirming its efficacy upon a more extended scale, and of ascertaining the efficient treatment of those cases in which magnesia is ineffectual, and in which it has even been found to aggravate the complaint.

To bring forward additional evidence in favour of the use of magnesia,

* From Phil. Trans. 1813. See Phil. Journal, XXVIII, 132.

† For 1813, p. 106.

had failed, and where the digestive organs had been injured in consequence of the use of such remedies: the time which has elapsed since the cure of this and other cases, without a relapse, is also strongly in favour of this mode of treatment.

Second case.

*Case* 2. A gentleman, twenty years of age, who had suffered from heartburn, and other dyspeptic symptoms, was seized, on the 1st of June, 1811, with a violent pain in the loins, and more especially in the right kidney; and during the night he passed a large quantity of red sand with his urine. On the 2d, with a view to relieve the pain, which had increased considerably, he took fifty drops of laudanum, and drank freely of barley water. The night was passed more quietly; but on the morning of the 3d, he was seized with a violent pain in the kidney, and with the usual symptoms of the passage of a calculous along the ureter. These continued, with more or less violence, till the evening of the 4th, when he became perfectly easy, and remained so till the morning of the sixth, when, with considerable pain and difficulty, he voided a calculous composed of uric acid, weighing nine grains. For several successive days his urine deposited a large quantity of red sand, and three very small round calculi were voided.

Calculus voided.

He was now directed to abstain from all kinds of fermented liquors and sour food, and to take a pint of treble soda water, (containing three drachms of sub-carbonate of soda,) daily. Under this treatment he continued to recover, and remained perfectly free from complaint until the end of August, when a copious deposit of red sand appeared in his urine: he had little pain in the affected kidney, but complained of almost constant nausea, or want of appetite. The soda water was increased to a pint and a half, and afterwards to two pints daily, and in the intervals, he drank very freely of barley water.

Trial of magnesia after other remedies.

Having persevered in this way for ten days without receiving any benefit, he was induced to make a trial of magnesia, of which he took one tea-spoonful night and morning in cold chamomile tea. In about a week the state of his stomach was much improved, and the deposit in the urine proportionally dimi-

diminished, and in three weeks every symptom of disease had disappeared.

In February, 1812, having persevered in the use of magnesia, with little intermission, I was informed that the sand had returned, that increasing the quantity of magnesia had produced no good effect, and that alkalies materially aggravated his complaint by disagreeing with the stomach, and greatly increasing the urinary deposit.

Magnesia improper when the deposit does not consist of uric acid.

On examining the sand, I found that, instead of consisting, as formerly, of uric acid, it was composed of a mixture of the ammoniaco-magnesian phosphate with phosphate of lime; he was directed to abstain from magnesia and alkalies, and to adopt a plan of treatment which it is the object of the second section of this paper more particularly to explain.

The foregoing is a well-marked case of uric gravel with a strong tendency to form calculi, materially relieved by the use of alkaline remedies: it illustrates their usual effects when carelessly persevered in, and shews the advantage with which magnesia may, in such instances, be employed: it also exhibits the effect of magnesia and the alkalies, in producing the deposit of *white sand* (or phosphates) in the urine, when the *red sand* (or uric acid) has been removed.

The cases which follow are selected, from among others, to explain the best mode of preventing the formation of white sand, and to shew the most effectual treatment where it is a natural deposit in the urine, or where it has been induced by the incautious exhibition of alkaline medicines.

## Section II.

Treatment when the deposit consists of am. magnes. phosphate.

The white sand so frequently voided by persons labouring under calculous complaints, was first analysed by Dr. Wollaston*, who found it composed of ammoniaco-magnesian phosphate, either alone or mixed with variable proportions of phosphate of lime. The use of acid medicines in

* Phil. Trans. 1797.

these

these cases was also first suggested by the same able chemist, but although his valuable observations have been before the public for nearly fifteen years, I am not aware that any experiments have been made to ascertain what acids are best calculated to produce the desired effect, or to illustrate their mode of action.

Since my former communication, I have lost no opportunity of attending to this important subject, and hope that the conclusions, suggested by the following cases, will be deemed satisfactory, and that their application in practice may lead to useful results.

Case.

*Case* 1. A gentleman, fifty years of age, who about ten years before had undergone the operation for the stone*, was attacked on the 14th January, 1810, with violent pain in the right kidney and ureter, which lasted two days; on the 17th, these symptoms subsided, and were followed by those of stone in the bladder, which continued for some days, and although he had taken abundance of barley water, and similar diluents, the stone shewed no disposition to pass. On account of his former sufferings, this circumstance rendered him extremely uneasy, and on the evening of the 21st, he suffered several severe paroxysms of pain, on attempting to make water. Under these circumstances, he was desired to take a purge, composed of two ounces of infusion of senna, two drachms of tincture of senna, and twenty grains of powdered jalap.† In three hours this began to take powerful effect, and during the violence of the operation, he was so fortunate as to void the calculus with his urine. It weighed eight grains. On the 28th,

* The stone extracted consisted of a nucleus of uric acid, about the size of a pea, incrusted with a mixture of the phosphates. It was broken during the operation, but appeared to have been of the size of a pigeon's egg.

† I recommended this treatment in consequence of having heard Sir Everard Home state a case, in his Surgical Lectures, of a gentleman who suffered a bougie to pass so far into the urethra, that it could not be removed by any instrument. During the operation of a purge it was expelled with considerable force.

he

he again suffered pain in the region of the kidneys, and voided much sand, composed of uric acid, with ammoniaco-magnesian phosphate. He now took three half pints of soda water daily, which materially increased the proportion of the triple phosphate, while that of uric acid was considerably diminished. Ten drops of muriatic acid were then taken three times a day in water. The red sand now began to re-appear, and on the 4th of February, he voided a very small uric calculus. The urine made after dinner contained more or less mucus, streaked with blood, a symptom which was much aggravated by a slight excess in wine. On the 6th, he left London, and employed no medicine until the 12th, when he returned, in consequence of having voided a large quantity of the white sand. Exhibition of acids.

Having observed the efficacy of carbonic acid in preventing the deposition of the phosphates, and having found it less liable than any other acid to induce a return of the uric gravel and calculi, I now directed him to take half a pint of water, highly impregnated with fixed air, four or five times a day, and to drink cider instead of wine. On the 18th of February, his urine was less turbid than it had been for some months before, and on the 20th of March, having continued the use of carbonic acid, he had no remaining symptoms*.

In August, his urine became again turbid, but by the use of vinegar and lemon juice at his meals, which acids, he now finds, have no tendency to induce a return of the red gravel, he succeeds in preventing this symptom.

*Case* 2. On the 11th of October, 1812, the operation for stone in the bladder was performed upon a boy, eleven years of age, and four calculi were extracted, of which the largest was of the size of a small horse bean: they were each composed Case 2. Deposition of am. mag. phosphate.

* I have several times examined the urine, with a view to ascertain whether any of the acids which were exhibited, could be detected in that secretion; but the results of such experiments are so much interfered with by the very compound nature of the urine, that I have not hitherto been able to draw any satisfactory conclusions respecting them.

of

of a nucleus or centre of uric acid, upon which the ammoniaco-magnesian phosphate was deposited.

After the operation, the urine deposited a large quantity of white sediment, and some small pieces of red gravel were occasionally voided. He was now directed to take eight grains of citric acid dissolved in barley water, three times daily; under this treatment, the sediment in the urine was considerably diminished, but did not wholly disappear. The dose of the acid was gradually increased to twenty grains, by which means the sediment was only occasionally deposited, and consisted of little else than mucus. It was observed, that whenever the citric acid was omitted, even for twenty-four hours, the sediment was greatly increased, and this was constantly attended with frequent desire to make water, and other symptoms of irritation in the bladder. On resuming the use of the citric acid, the sediment always disappeared, and the irritation of the bladder subsided, and this happened so frequently, that no doubt could be entertained of the influence of the medicine on the composition of the urine.

**Beneficial use of citric acid.**

This plan of treatment was continued for three months; at the end of that period, it was found that the urine had not the same disposition to deposit the phosphates as formerly; even when the medicine was omitted, the sediment was small in quantity, and not constant in its appearance. He was now directed to omit the use of the citric acid, and occasionally to eat oranges and other acid fruits. He continued this plan until the beginning of April, 1813; his urine was then quite clear, and he had no symptoms of disease.

Case 3. Of the white deposit removed by the careful use of acids.

*Case* 3. In the month of October, 1811, a gentleman, thirty-four years of age, informed me, that he had observed a white deposit in his urine, during the whole of the preceding summer. He had taken considerable quantities of soda water, which he thought increased the sediment, and alkalies in any other form produced a very obvious aggravation of the complaint.

His urine was at all times clear when voided; but after a few hours, a white powder was observed to separate from it, and a film of crystalline matter formed upon the surface. The former

former consisted of phosphate of lime and mucus, the latter of the ammoniaco-magnesian phosphate.

He was directed to take one drachm of muriatic acid properly diluted, at divided doses, during the day; and it was proposed that he should pursue this plan for a week; but it was discontinued on the third day on account of its acting upon the bowels, and producing a frequent desire to make water*.

On the 10th of October, he was advised to take two large glasses of lemonade daily, and to substitute claret for port wine, a pint of which he was in the habit of drinking daily. Under this treatment the symptoms produced by the muriatic acid subsided; but the appearance of the urine was not at first improved.

On the 20th, the film of triple phosphate formerly constantly observed in the urine began to decrease, but the white sand remained as abundant as before; he was therefore directed to take twenty grains of citric acid twice a day, and to continue the use of acid drink, as formerly.

The additional acid at first disagreed with the bowels; but this effect soon ceased, and the sediment was only observed in the urine voided in the morning; he therefore took another dose of the acid every night. This plan was pursued with little intermission until the beginning of December: the deposition of the phosphates gradually ceased, and he remained in perfect health until the middle of May, 1812, when after violent exercise, and taking more wine than usual, the white sand again made its appearance in great abundance; his stomach became extremely irritable, and the acids, which he had before employed with success, brought on considerable irritation in the bladder. The addition of ten drops of laudanum to each dose of the citric acid prevented this effect, and he was thus enabled to continue the acid, which in a fortnight relieved his complaint.

* In this and other instances the sulphuric and nitric acids were occasionally substituted for the muriatic; but they were found equally inadmissible.

This

This gentleman informed me, that whenever he omitted the use of an acid diet, or took much wine, especially port, his urine deposited the white sand and mucus, for two or three successive days.

Case 4. A very aged patient relieved by similar treatment.

*Case* 4. A gentleman, eighty years of age, who had twice submitted to the operation for the stone within five years, voided with his urine considerable quantities of white sand and mucus.

From the age of this patient, and the account of his case, there appeared little doubt that the calculi had been formed in consequence of a diseased prostate gland, in the manner described by Sir Everard Home*, and on examining them, they were found to contain no uric nucleus, nor indeed had there been any symptoms of disease in the kidneys, at any previous period.

This gentleman had been in the habit of taking soda water, from which he was now desired to abstain, with a view of putting him upon the acid plan of treatment. He was ordered to take eight drops of muriatic acid three times a day in two table-spoonsfuls of water; but the third dose produced so much irritation in the bladder, and consequent increase of his symptoms, that it became necessary to adopt another treatment.

Lemon juice, or a solution of the pure citric acid, when given in quantity sufficient to produce any change in the appearance of the urine, had the same effect as the muriatic acid.

As water impregnated with carbonic acid could not be procured, he was directed to dissolve, in separate portions of water, twenty grains of citric acid, and thirty grains of the crystallised carbonate of potash, and to take the mixed solutions, during the effervescence. This quantity was at first only taken night and morning, but as it agreed perfectly well, it was afterwards repeated four or five times daily. Under these circumstances the appearance of the urine was soon improved,

* Practical Observations on the Treatment of Diseases of the Prostrate Glands, p. 39.

and

and both the mucus and the sand were considerably diminished in quantity. In six weeks, the urine, when voided, was transparent; but a considerable deposition of the phosphates took place, when it had remained some hours at rest. In this state he left London, and has since informed me, that the sediment gradually diminished under the use of the carbonic acid, that his urine is never turbid, and that the irritation in the bladder has entirely subsided.

It did not appear necessary to detail the minutiæ of the above cases; they have been selected with a view to elucidate the treatment of the disease, as far as it depends upon chemical principles, and to furnish the data upon which the following conclusions are founded. Summary.

1. That where alkalies fail to relieve the increased secretion of uric acid, and to prevent its forming calculi in the kidneys, or where they disagree with the stomach, magnesia is generally effectual, and that it may be persevered in for a considerable time without inconvenience, where the tendency to form excesss of uric acid remains.

2. When the alkalies, or magnesia, are improperly continued, after having relieved the symptoms connected with the formation of the red sand, or uric acid, the urine acquires a tendency to deposit the white sand consisting of the ammoniaco-magnesian phosphate and phosphate of lime.

3. The mineral acids, (muriatic, sulphuric, and nitric) diminish, or entirely prevent the deposition of the phosphates; but are apt to induce a return of the red gravel.

4. That vegetable acids, especially the citric and tartaric, are less liable to produce the last mentioned effects, even when taken in large doses for a long time; and that carbonic acid is particularly useful in cases, where the irritable state of the bladder prevents the exibition of other remedies.

## II.

*On the Measure of Moving Force. By Mr.* PETER EWART*.

(*Continued from p. 66.*)

Cases of difficulty in the doctrines of moving force.

10. If two elastic balls equal balls E and F, (fig. 10.) moving with the respective velocities A C and A B, at right angles to each other, strike at the same instant a third elastic ball A, equal to E or F; E and F will be brought to rest, and A will move off with the velocity and in the direction A D. In this case, the whole amount of the forces of E and F must have been communicated to A; but the velocity acquired by A is less than the sum of the velocities of E and F.

11. If the directions of E and F be not at right angles, and if $AC = AB$ (as in fig. 11.) the result will be as follows: produce A B, and draw the perpendicular D G after the stroke, the velocity of A in the direction A B, will be $\frac{2\,AB \times AD}{AB + AG}$, and E and F will each continue to move in their first directions with the velocity $\frac{AB \times BG}{AB + AG}$*.

In this case, as in all others, the velocity and the direction of the centre of gravity of the system is, no doubt, the same before and after collision. But that is only one feature of the case. If we examine all the results after collision, we shall find that the motion of A is not the same as it would have been if it had been struck by a mass equal to $E+F$, having the same velocity as the common centre of gravity of E and F before collision. If, however, we reckon the forces as the masses into the squares of their absolute velocities, we shall (if they be perfectly elastic) always find that whatever force is lost by the striking balls, is gained by that which is struck.

12. Let four equal balls A, B, D, E, (fig. 12.) revolve about their common centre of gravity, C. Let A and B be

* If BAC be an obtuse angle, the same solution applies, only E and F rebound instead of proceeding forward.

connected

Cases of difficulty in the doctrines of moving force.

connected by a rod of no sensible inertia, and D and E by a similar rod, but unconnected with A and B. Let the distance of the centres of gyration of A and B be twice that of D and E, and let D and E make two revolutions while A and B make one. If the balls and rods be elastic, and the velocity of each ball 10, and if the rod connecting A and B be struck by the balls D and E at their centre of gyration, the velocity of A and B after the stroke will be 14, and that of D and E will be 2. If the balls and rods be non-elastic, the velocity of A and B after the stroke will be 12 and that of D and E, 6.

In the first case, the sum of the products of the masses into the squares of their respective velocities, is the same before and after collision; but in the second case, that sum is less after than before collision; and it must, I presume, be admitted, that the rotatory force in that case is diminished by the collision.

13. If an iron prism A B, (fig. 13) moveable on a fixt centre at A, be let fall on a piece of soft clay C, the greatest impression might be expected to be made when the clay is placed under P, the centre of percussion of the prism. But if the experiment be made, the impression will be found to be the same, whether the clay be placed at C, D, or E, or at any other distance from the centre of motion.

14. Let two equal elastic balls A and B, (fig. 14.) be connected by an elastic rod, and be at rest in free space, and let G be their common centre of gravity. If another elastic ball C, whose mass is equal to the joint masses of A, B, and the rod, moving with the velocity $v$ in the direction C G at right angles to the rod, strike it at G; C will be brought to rest, and G will move off with the velocity $v$, in the direction C G. But if we repeat the experiment, applying the force of D instead of C, the mass of D being equal to that of A or B and half the rod; and its velocity equal $2v$, striking A at its centre of gyration round the point G, the result will be as follows: D will be brought to rest, G will move off as before with the velocity $v$, and A and B will have a rotatory motion about g, with the velocity $v$ at their centres of gyration. In

both

Cases of difficulty in the doctrines of moving force.

both instances, the striking forces, if measured by the masses into their velocities, are the same; and as the striking balls are in both instances brought to rest, they must have communicated exactly their whole force to the mass which was struck. The results, however, are far from being equal. If the force of D be no greater than that of C, we shall have the rotatory motion produced without force, although we have no reason to suppose that the rotatory can be produced with less force than the rectilinear motion.

In order to avoid unnecessary calculations or analyses, I have stated these cases in the most simple forms I could devise. I am aware, that there are many who think they may be easily solved in the usual way, and that some of the cases will be considered as trivial paradoxes. But if we examine the explanations which have been given of similar cases, we shall find that there is considerable diversity of opinion about the principles by which they are to be explained; and that some of the solutions are not quite so obvious as, at first sight, they appear to be.

Before we enter upon the examination of these particular cases, it may be proper to observe, in addition to what has been already noticed, that, in respect to the general question, or in respect to the existence even of any question at all on this subject, some of the best recent authorities are the most difficult to be reconciled with each other.

Few authors, in our language, on the principles of mechanics, have been more generally read and referred to than Emerson. From the great analytical skill of this author, one would have expected something decisive on the long-pending question concerning the measure of moving force; but he seems to take for granted, that the measure is the mass into the velocity or the *momentum*, for he scarcely condescends to mention the other, and, after a few observations, dismisses it in the following laconic manner. " It seems to be a necessary property of the *vis viva*, that the resistance is uniform. But there are infinite cases where this does not happen; and, in such cases, this law of the *vis viva* must fail. And since it fails in so

many

many cases, and is so obscure in itself, it ought to be weeded out, and not to pass for a principle in mechanics*."

Cases of difficulty in the doctrines of moving force.

Mr. Atwood, however, has shewn, that Mr. Emerson himself has been led into error by neglecting this very principle which he proposes to weed out. In reference to a particular problem, he says, "In Emerson's Fluxions, p. 177†, there is this problem: The radii of a wheel and axle are given in the proportion of $b:a$; a weight $w$ acting by means of a line on the circumference of the wheel, elevates a weight $y$ suspended from a line which goes round the axle; it is required to assign the quantity $y$, when $y$ × into its velocity generated in a given time, is the greatest possible."

"In the solution, the author supposes the momentum of bodies to be as the quantity of matter into the velocity generated; and, according to the usual doctrine of momentum, assumes it as an universal truth, that if a force acts on any different quantities of matter for a given time, it will always generate the same moment, estimated by the quantity of matter into the velocity. From this reasoning he deduces the weight $y=\sqrt{\overline{2}-1}\times\frac{bw}{a}$ when its true value is $y=w\times\sqrt{\frac{b^4}{a^4}+\frac{b^3}{a^3}-\frac{b^2}{a^2}}$ (page 249.) agreeing with the former only in the extreme case, when $b=a$, that is, when the radius of the wheel is equal to that of the axle‡."

Mr. Smeaton, at the commencement of the description of his experiments on water-wheels, says, "The word *power*, as used in practical mechanics, I apprehend to signify the exertion of strength, gravitation, impulse, or pressure, so as to produce motion§." And near the end of his *Experimental Examination*, we have the following conclusion:

"It therefore directly follows, conformably to what has been deduced from the experiments, that the mechanic power that

* Emerson's Principles of Mechanics, p. 20.

† Second Edition.

‡ On Rectilineal Motion, Preface, p. x.

§ Phil. Trans. 1795, p. 105.

must,

Cases of difficulty in the doctrines of moving force.

must, of necessity, be employed in giving different degrees of velocity to the same body, must be as the square of that velocity." And in the next page he observes, " It seems, therefore, that without taking in the collateral circumstances both of time and space, the terms quantity of motion, *momentum,* and force of bodies in motion, are absolutely indefinite ; and that they cannot be so easily, distinctly, and fundamentally compared, as by having recourse to the common measure, viz. mechanic power*."

M. De Prony, however, gives a different conclusion, as follows : " Il y a eu des disputes très vives parmi les mathématiciens pour savoir si on devoit faire la *force* d'un corps en mouvement proportionelle à la vîtesse ou au quarré de la vitesse : il est bien aisé, d'après tout ce qui précede, de réduire la question à un énoncé raisonnable qui en suggérera sur-le-champ la solution. Le mot *force* ne désignant qu'une cause dont la nature est inconnue, et dont les effets sont les seules choses que nous puissions mesurer, il est clair qui par ce mot *mesure de la force,* on ne peut entendre que celle d'un de ses effets ; or ces effets pouvant se considérer sous différents aspects, chacun comporte une espece de mesure particuliere et conforme à sa nature. Cela posé, si l'on considere l'effet de la force comme consistant dans la destruction d'une certaine sommes d'obstacles ou de quantités de mouvement, cette somme est proportionnelle à la simple vîtesse. Si on ne considere point l'effet de la force relativement à la somme des obstacles vaincus, mais relativement à leur nombre, ce nombre sera proportionnelle au quarré de la vîtesse lorsque tous les obstacles seront égaux†.

" On

* Phil. Trans. 1776, p. 473.

† What is here meant by the *sum* and *number* of obstacles, is not very obvious. That explanation has, however, been adopted by various other authors. It appears to have originated with D'Alembert, who states it thus : " Donc dans l'équilibre le produit de la masse par la vitesse, ou ce qui est le meme chose, la quantité de mouvement, peut représenter la force. Tout le monde convient aussi que dans le mouvement retardé, le nombre des obstacles vaincus est comme le quarré de la vitesse ; ensorte qu'un corps qui a fermé un ressort, par exemple,

Cases of difficulty in the doctrines of moving force.

"On voit par-là que la fameuse question des *forces vives* n'est qu'une dispute de mots qui n'auroit jamais subsisté si l'on avoit voulu s'ententre, c'est à dire analyser et définir†."

On the other hand, Dr. Milner, of Cambridge, holds, "that it is plain, that if any one contends for the equality of action and reaction, and explains those terms by the change produced in the absolute forces of bodies, the dispute is not merely verbal‡." And again he says, "some writers have considered this question as entirely verbal, and have affected to treat the advocates on both sides with the greatest contempt. Such persons save themselves a great deal of trouble, and have the credit of seeing farther into the controversy than others; but, after all, I am afraid the practical mechanic will receive little information or security from such speculations§."

exemple, avec une certaine vitesse, pourra avec une vitesse double fermer, ou tout à la fois, ou successivement, non pas deux, mais quatre ressorts semblables au premiere, neuf avec un vitesse triple, et ainsi du reste."—"Il faut avouer cependant, que l'opinion de ceux qui regardent la force comme le produit de la masse par la vitesse, peut avoir lieu non-seulement dans le cas de l'équilibre; mais aussi dans celui du mouvement retardé, si dans ce dernier cas on mesure la force non par la quantité absolue des obstacles, mais par la somme des resistances de ces mêmes obstacles. Car on ne sauroit douter que cette somme de résistances ne soit proportionnelle à la quantité de mouvement, puisque, de l'aveu de tout le monde, la quantité de mouvement qui le corps perd à chaque instant, est proportionnelle au produit de la resistance par la durée infiniment petite de l'instant, et que la somme de ces produits est évidemment la résistance totale. Toute la difficulté se reduit donc à savoir si on doit mesurer la force par la quantité absolue des obstaeles, ou par la somme de leurs résistances. Il paroitroit plus naturel de mesurer la force de cette dernier maniere, &c*." But it should be remarked, that although equal quantities of motion are lost in equal times, it is not universally acknowledged that these equal times denote equal quantities of force, or equal quantities of resistance. That, indeed, is the very question at issue.

* Traité de Dynamique Discours Prelim. p. 20 et 21.

† Arch. Hydr. p. 24.

‡ Phil. Trans. 1778, p. 377.

§ Ibid. p. 378.

Cases of difficulty in the doctrines of moving force.

Dr. Wollaston's opinion is, that "the conception of a quantity dependent on the continuance of a given *vis motrix* for a certain *time* may have its use, when correctly applied, in certain philosophical considerations; but the idea of a quantity resulting from the same force exerted through a determinate *space* is of greater practical utility, as it occurs daily in the usual occupations of men*." And he concludes his lecture on the force of percussion thus: "In short, whether we are considering the sources of extended exertion, or of accumulated energy, whether we compare the accumulated forces themselves by their gradual or by their sudden effects, the idea of mechanic force in practice is always the same, and is proportional to the *space* through which any moving force is exerted or overcome, or to the *square* of the velocity of a body in which such force is accumulated." This conclusion coincides nearly with Mr. Smeaton's, but still it remains to be explained how two given quantities of force may, consistently, be considered as equal to each other for philosophical purposes, but unequal for all practical purposes.

The Edinburgh reviewers of Dr. Wollaston's lecture adopt a different doctrine. In reference to the first passage quoted above, they say, "Now, with the judgment here given as to the respective utility of the two measures of the force of moving bodies, we cannot entirely agree, though we differ from Dr. Wollaston with considerable diffidence; and the more, that his opinion is supported by one of the greatest authorities in practical mechanics, of which this or any other country can boast—the late Mr. Smeaton†." And after some remarks on supposed errors of Mr. Smeaton, which I shall have occasion to refer to again, they say, "To whatever cause, therefore, the imperfection of the theory of the machines moved by water is to be ascribed, it is not to any thing that would be corrected by the introduction of a measure of force different from that which is commonly in use‡." At the beginning, however, of the same ar-

* Phil. Trans. 1806, p. 15.

† Edinburgh Review, vol. 12, p. 122

‡ Ibid, p. 126.

ticle,

ticle, they give the following opinion : "It is no longer doubted, that this force (of percussion) may, with perfect truth, be considered as proportional, either to the quantity of matter multiplied into the velocity, or to the quantity of matter multiplied into the square of the velocity, according to the nature of the effect which it is destined to produce*."

Cases of difficulty in the doctrines of moving force.

On the subject of forces, M. Laplace expresses himself as follows : "La force peut être exprimée par une infinité de fonctions de la vîtesse, qui n'impliquent point contradiction. Il n'y en a point, par exemple, à la supposer proportionnelle au carré pe la vitesse†." After stating a hypothetical example of force, where the results would be different from those of experience, but where the square of the velocity is taken in a sense quite different from that in which it appears to have been understood by every other author I have had an opportunity of consulting, he proceeds : "Parmi toutes les fonctions mathématiquement possibles, examinons quelle est cello de la nature." And, after reasoning at some length on various effects of force, he concludes, "Voila donc deux lois du mouvement, savoir, la lois d'inertie et celle de la force proportionnelle à la vîtesse, qui sont données par l'observation. Elles sont les plus naturelles et les plus simples que l'on puisse imaginer, et sans doute, elles dérivent de la nature meme de la matière ; mais cette nature étant inconnue, ces lois ne sont pour nous, que des faits observés, les seuls, au reste, que la mécanique emprunte de l'expérience‡."

It appears, then, to be the opinion of this distinguished philosopher, that, although it may be mathematically possible for the force of a body in motion to be proportional to the square of its velocity, yet such a principle is inconsistent with the phenomena of nature ; but that the law of inertia and the law of force proportional to the velocity, are the most natural and the most simple principles imaginable, that they are derived

* Edinburgh Review, vol. 12, p. 1:6.

† Système du Monde, 3d. edit. Liv. III. ch. 2. p. 141.

‡ Ibid. p. 144.



from

Cases of difficulty in the doctrines of moving force.

from the very nature of matter, and that they are the only facts which the science of mechanics borrows from experience.

It may be proper to observe here, that M. Laplace adopts as first principles, only the two first of Sir Isaac Newton's laws of motion.

It is surprising, that so many different opinions on this subject should still be held, and it is not easy to understand how so many good reasoners have, from the same data, drawn conclusions so much at variance with each other.

Fifty years ago, M. D'Alembert, speaking of the science of mechanics, observed, that " En général, on a été plus occupé jusqu'à présent à augmenter l'édifice qu'à en éclairer l'entrée ; et on a pensé principalement à l'élever, sans donner à ses fondemens toute la solidité convenable*."

No one will deny that, during the last fifty years, great advances have been made in the application of mechanical principles to the investigation of the motions of the heavenly bodies. But as far as these principles have been adapted to practical uses, may not M. D'Alembert's observation be, with some justice, applied to the present state of mechanical science ? or, may it not be said that, not only the entrance, but the interior of the structure is not very conveniently arranged for the occupations of life.

But there is another observation of M. D'Alembert, which has, on the present occasion, still stronger claims on *my* attention. He says, " mais il semble que la plûpart de ceux qui ont traité la question de la mesure des forces, ayent craint de la traiter en peu de mots."

Although the censure be severe, it may be just, and I shall endeavour to profit by it. Some repetitions, however, in discussions of this kind are unavoidable.

In the observations which I have made, as well as in those which I have yet to make, on various passages in some of the best authors on mechanics, I hope to escape the charge of being, in any degree, disrespectful towards them. I am sensible,

* Traité de Dynamique, Discours prelim. p. 4.

that

Cases of difficulty in the doctrines of moving force.

that any remarks, having that tendency, would ill become me, and could be of no avail in my argument. Anxious as I am to state distinctly the reasoning and the conclusions which appear to me to be objectionable, I am not less anxious to state them fairly and respectfully. I am well aware of the disadvantages under which I labour; the general prejudice against this subject being so strong, that a great national institution has absolutely proscribed the discussion of it*. That circumstance, however, enhances the value of the indulgence of which I now avail myself, in submitting it to the consideration of this society.

Proceeding now to the consideration of the particular cases which I have described, I may observe, that the first two cases comprehend, I believe, the chief points at issue, as far as they relate to force producing rectilinear motion by the intervention of levers or wheels, and to motion produced about fixed axles.

That the forces of C and D, in the first case, are equal, cannot, I think, be questioned; and it is not less obvious that their effect, if estimated by the masses into the squares of their velocities are also equal.

In the second case, the force of P is twice that of Q, and the effect of these forces, if measured by the masses into the squares of their velocities, are respectively in the same proportion.

Mr. Atwood, (as we have already noticed at page 109) admits, that the measure composed of the mass into the square of its velocity obtains in all cases of rotatory motion about fixed axes; and that the measure composed of the mass into its velocity, when applied to the same cases, fails; "a given quantity of motion thus estimated, being alterable in any assigned ratio."

But authors on mechanics generally concur in the following conclusion: that "a distinction is always to be made between the actions of bodies when at liberty, and when they

† The French National Institute has, I understand, prohibited the reception of all dissertations on the measure of force.

revolve

Cases of difficulty in the doctrines of moving force.

revolve about a centre or axis. In the first case, the motion lost is always equal to the motion communicated in an opposite direction: in the second, the motion lost is to be increased or diminished in the ratio of the levers before it will be equal to the motion communicated*."

We do not find, however, that the absolute forces or their effects, can be increased or diminished by any alteration in the lengths of the levers. For if the arm HG, for example, be extended to any assumed length, the same velocity will still be produced in *m* by the motion of P through the same space. It is true the velocity will not be produced in the same time; but the result will be the same, in whatever time, or by whatever complication of levers or wheels, it may be produced.

The converse of this case is stated by Dr. Wollaston as follows: "It may be of use also to consider another application of the same energy, and to shew more generally, that the same quantity of total effect would be the consequence not only of direct action of bodies upon each other, but also of their indirect action through the medium of any mechanical advantage or disadvantage; although the time of action might, by that means, be increased or decreased in any desired proportion. For instance, if the body supposed to be in motion were to act by means of a lever upon a spring placed at a certain distance from the centre of motion, the retarding force opposed to it be inversely, as the distance of the body from the centre; and since the space through which the body would move to lose its whole velocity would be reciprocally as the retarding force, the angular motion of the lever and space through which the spring must bend, would be the same, at whatever point of the lever the body acted†." Practical men are much beholden to Dr. Wollaston. He is, I believe, the only author, professedly on the theoretical principles of mechanics, who has written decidedly in support of Mr. Smeaton's conclusions; and we have only to regret, that he has not pursued the subject farther.

* Dr. Milner. Phil. Trans. 1776, p. 371.

† Phil. Trans. 1806, p. 21.

If

If the amount of the force could be increased or diminished by any variation of the length of the lever, we might expect to find its measure to be of that indefinite kind which might be estimated by the product of the mass into *any function* of its *velocity*. Such a conclusion, however, is quite inconsistent with experience; for under every variation of the proportions of the lever, the effect, if measured by the mass into the square of its velocity, is uniformly found to be in proportion to the moving force by which it is produced; if that force be measured by the pressure multiplied into the space through which it acts. But if we multiply the mass into any other function than the square of its velocity, no such general correspondence between the force and its effects is to be found.

Cases of difficulty in the doctrines of moving force.

Mr. Smeaton has well illustrated this principle by many valuable experiments on the more complicated cases of the action of water on mill-wheels, and on force generating rotatory motion in masses of matter about fixed axes*.

The Edinburgh reviewers of Dr. Wollaston's lecture on the force of percussion have urged some strong objections against Mr. Smeaton's conclusions. I would willingly excuse myself from venturing to controvert any thing in a criticism written with so much candour and ability; but some of the arguments it contains are pressed so powerfully against the application of the square of the velocity of a body in motion as the measure of its force, that they must, I apprehend, be answered before that measure can be consistently defended.

In the first case it is argued, that the principle which Mr. Smeaton understood to be confirmed by the result of all his experiments, " is, in fact, abandoned by him at the very outset of his investigation, in consequence of having included the time in the measure of the effect*." Now, I do not see how this supposed contradiction in Mr. Smeaton's reasoning can possibly be maintained. The measure of mechanical power adopted by him consists of the pressure multiplied into the space through which it acts. In cases where the pressure moves

* See Phil. Trans. for 1759 and 1776.

† Edinburgh Review, vol. 12, p. 123.

through

Cases of difficulty in the doctrines of moving force.

through equal spaces in equal times, it can make no difference whether the *time* or the *space* be taken as an element of the mechanical power; and when, in such cases, Mr. Smeaton takes either of these, it does not follow that he abandons the other.

He does not say that the consideration of the time is necessarily excluded; he only says it is not necessarily included in the estimation of mechanical power; and he has, at the conclusion of the passage referred to by the reviewers, taken care to discriminate the particular cases in which the time may or may not be so taken into consideration. He says, "but *note* all this, (relating to the quantity of power expended in raising a known weight with an uniform velocity to a known height) as to be understood in the case of slow or equable motions of the body raised; for, in quick, accelerated, or retarded motion, the *vis inertia* of the body moved will make a variation*."

He might, indeed, consistently with his principles, have excluded altogether the consideration of the time in which any mechanical effect is produced. For, according to these principles, the same quantity of mechanical power is required to raise a given weight to a given height, in whatever time it may be effected, or whether the motion be equable or not, *provided that the velocity of the weight at the beginning and the end of the operation be the same*†. Accordingly he says, "from the whole of what has been investigated, it therefore appears, that time, properly speaking, has nothing to do with the production of mechanical effects, otherwise than as, by equally flowing, it becomes a common measure; so that whatever mechanical effect is found to be produced in a given time, the uniform continuance of the same mechanical power will, in a double time, produce two such effects, or twice that effect. A mechanical

* Phil. Trans. 1759, p. 106.

† It is, I presume, hardly necessary to say, that when the motion of the weight is so quick as to make the resistance of the air or any other medium through which it moves, considerable; other effects, besides the mere raising of the weight, must be taken into the account.

nical power, therefore, properly speaking, is measured by the whole of the mechanical effect produced, whether that effect is produced in a greater or lesser time*." From the context it is obvious, that by "*the uniform continuance of the same mechanical power*," he means a continuance of an uniform pressure moving through equal spaces in equal times, and he considers that to be a perfect uniformity of action.

Cases of difficulty in the doctrines of moving force.

(*To be continued*)

---

III.

*The Theories of the Excitement of Galvanic Electricity explained in Mr. William Henry's Paper, compared with the Phenomena of the Electric Column. By J. A.* De Luc, *F. R. S. &c.*

*To William Nicholson, Esq.*

SIR,

YOUR valuable Journal is become the repository of the progress of the electric science, and of the opinions on the agency of the electric fluid in various phenomena. In particular you have favoured me with the publication of many of my papers concerning that science, to which papers I must first refer, as a necessary introduction to the subject announced in the above title.

Excitement of galvanic electricity, and phenomena of the electric column.

Your number for June, 1810, contained the first part of my Analysis of the galvanic Pile. Having followed this analysis by a long series of experiments, I discovered in this apparatus two different operations which deserved to be thoroughly investigated.

The experiments which I had made for the purpose of following up this first step, were the subject of my paper in your number for August, in the same year. These experiments, made on the galvanic pile itself, manifested it clearly in two distinct operations; one, from which originate all the effects of that apparatus, is to set in motion a certain quantity of electric fluid, which motion is produced simply by the association, in

* Phil. Trans. 1776, p. 473.

suc-

Excitement of galvanic electricity, and phenomena of the electric column.

successive pairs, of zinc and copper, or silver plates; these pairs being separated by some non-metallic conducting substance. The other operation, entirely distinct from the former, although owing to it its origin, is to produce chemical effects: and this depends on some liquid being interposed between the metals in each pair; whence results a particular modification of the electric fluid.

Having discovered, by the phenomena of the galvanic pile itself, these two distinct functions, I conceived that they might be separated in an apparatus which, retaining the faculty of setting in motion the electric fluid, should not produce any chemical effect, since nothing was required but to suppress the liquid. I made the trial, and this is the origin of the apparatus, which I have named electric column, the effect of which is to set the electric fluid in motion, without any chemical effects, either in itself, or between its extremities. These preliminaries are, I think, sufficient to introduce the subjects announced in the title of this paper.

Though the electric column, as a new electric apparatus, was, in my opinion, deserving the notice of experimental philosophers, I did not flatter myself to see it so much enlarged and improved as I have been first informed by your Journal of June last, in which I found Mr. George John Singer's paper on his electric column, with an account of its astonishing phenomena. I could not, therefore, but be surprised to find, two months after, in your Journal for August, the paper of Dr. William Henry, treating of the theories of excitement of galvanic electricity, in which not only no mention is made of Mr. Singer's electric column and its effects, but theories are successively offered, which ought to have been prevented by the description of the phenomena of the new apparatus. I must, therefore, suppose, that Mr. Henry, when he sent you his paper, had not any knowledge of those on the electric column preceding it, in your Journal ever since 1810; and I know, from circumstances communicated to me by Mr. Singer himself, that when Mr. Henry wrote his paper, he could not know the existence of his column. Therefore, after having detailed, in

in the following pages, the new light which Mr. Singer's apparatus has thrown on this subject, I shall have very little to add for pointing out the error of all the theories of the excitement of galvanic electricity, which are the object of Mr. Henry's paper.

Excitement of galvanic electricity, and phenomena of the electric column.

Mr. Singer had not been inattentive to what I had published in your Journal concerning my new apparatus; for he thus begins his paper: "The remarkable property of the electric column invented by Mr. De Luc, rendered the construction of that instrument on an extensive scale a desirable object. Trials were previously made on the effects of various combinations, to ascertain the most efficient arrangement."

Though I was struck by the title of Mr. Singer's paper, on the effect of twenty thousand zinc and silver plates, arranged as an electric column, which expressed an increase of the apparatus much beyond my expectation, I was still more astonished by the increase of the effect, so much they exceeded what I could have supposed of the mere increase of the number of pairs. This made me wish to have a direct intercourse with Mr. Singer; and, having obtained it, to my great satisfaction, I can now, Sir, explain to you what, though the inventor of the instrument, I could not understand myself in reading his paper; because, in describing the effects of his apparatus, he has not insisted on the particulars from which depend its astonishing power. And as the causes of that increase of power are peculiarly important in the electric science, they cannot but interest the more attentive part of your readers.

The principal changes made by this distinguished experimental philosopher in the electric column are two, one of which only is mentioned in his description, and of this I shall first speak: for which purpose I must return to the steps which led me to that apparatus, as I shall then better explain Mr. Singer's great improvement in that respect.

The first of these steps is expressed at the end of my paper in your number for June, 1810, concerning the galvanic pile. From the experiments which I before related, I arrived to this conclusion—that the electromotion in that instrument was produced

Excitement of galvanic electricity, and phenomena of the electric column.

duced by the mere association of zinc and silver, or copper plates, separated by a piece of paper; that this motion is proportionally greater when no liquid is used in the apparatus; but that no chemical effect was then produced. Whence I concluded, that a kind of pile might be made, producing electromotion, and no chemical effect.

My following paper, in your Journal for August, related to another class of experiments made with the same view. It came into my mind to try whether there would be some advantage to bring the paper closer to one of the metal plates, by pasting it on that metal. This idea led me to a great number of experiments with different metals, the general result of which was, that a great increase of electromotion actually does arise from pasting the paper on that one of the metals, which, in its association with the other, yields to it some electric fluid, as it is the case of copper and silver, which yield some electric fluid to zinc.

I have made various trials for the execution of my devised apparatus, by pasting paper on silver, and also on copper plates; but as it was with the view of a great increase of number, I was discouraged by the time and expence it would require, when luckily it came to my recollection, that a paper was fabricated in Germany, called in English Dutch-gilt paper, which afforded together a copper lamina to be applied to a zinc plate, as well as the paper necessary for its separation from the next zinc plate, and that the coppered surface of that paper was covered with a kind of varnish which preserves its brightness. I therefore procured some of that paper, and for a trial I constructed a pile composed of my large zinc plates, and of equal pieces of that paper, which exceeded my expectation, so great was the electromotion which it produced, comparatively to a pile of the same number of pairs in which the same metals were separated by a piece of loose paper.

The large size, however, of the parts of this first apparatus, and the difficulty of cutting them perfectly equal, was an impediment to that great increase of the number which I had in view. That consideration induced me to make the pieces much smaller,

in

in order that they might be cut with a punch. The instrument of this kind which I used was 0'7 inch in diameter, and there was also a contrivance for striking a small hole in the centre, in order to string them in pairs with a silk thread, in the form of a chaplet. Forming then two chaplets with all the plates which I had at that time, and associating them by their opposite sides with respect to positive and negative, they acted as if they had formed only one series. I therefore saw no limit to the enlargement of that natural electric machine; but other avocations prevented me from proceeding any further than the electric column of 600 pairs of that size described in your number for October, 1810, with the detail of the experiments, which I had made with that new apparatus.

Excitement of galvanic electricity, and phenomena of the electric column.

Such is the instrument that Mr. Singer has enlarged much beyond my expectation in every respect; but before I explain the causes of the increase of its effects, I must extract from his paper one of the most direct proofs that his apparatus acts as a natural electric machine, producing a surprising electromotion, yet unattended with any chemical effects, either between its extremities, or in itself.

With respect to the electromotion; when Mr. Singer applies his column to charge a Leyden jar containing about 50 square inches of coated surface, its discharge through our body is such, that it gives a shock sensibly felt in the shoulders, and by some individuals across the breast. This being the same effect produced by the common electric machine charging a Leyden jar, there cannot be any doubt that the fluid set in motion by the electric column, is the electric fluid.

I come now to the proofs, that the electromotion in the galvanic pile, the cause of which I have proved to be the same as in the electric column, is totally independent on any chemical agency, or any chemical effects; which characters of the galvanic pile depend only on a liquid being introduced between the zinc and silver, or copper plates. No liquid being used in the column, it must, therefore, be divested, in the first place, of producing chemical effects between its extremities. Mr. Singer has submitted this conclusion to experiments, respecting which

Excitement of galvanic electricity, and phenomena of the electric column.

which he says, towards the end of his paper, "Various saline compounds, tinged with the most delicate vegetable colours, have been made the medium of communication between the extremities, and the contact preserved many days: similar experiments have been made with metallic solutions; but in none of these trials have the smallest trace of chemical effect appeared."

There cannot be a more peremptory proof of the error of those theories detailed in Mr. Henry's paper, founded on the hypothesis, that the electromotion in the galvanic pile depends on its chemical effects. But there is another circumstance mentioned by Mr. Singer, which, at the same time that it corroborates that proof, exhibits the true nature of this natural electric machine, proves also, that the electromotion in it does not depend on any internal chemical effect. He says, in the conclusion of his paper,—"The cause of the electric excitement in the column appears to be permanent. I have some that have been now constructed upwards of two years, and their power is no way diminished: in the cases when the contrary has happened, I conceive the presence of too much moisture, and the consequent oxidation of the zinc surface, must have been the deteriorating cause."

This is an immediate proof that the presence of a liquid is not necessary to the electromotion in the galvanic pile itself; since, on the contrary, too much moisture in the electric column, and the consequent oxidation of the zinc surface, is a cause of diminution. This I have found in the course of my experiments on the galvanic pile; for the electromotion ceases in it, and consequently all the other effects, when the surface of all the zinc plates is deeply oxidated: so far from that chemical effect being the cause of the electromotion in the galvanic pile.

I must mention here, that, in speaking of galvanic phenomena, I refer them to their origin, the galvanic pile, as invented by Volta, not to the apparatus which is now much used, the galvanic troughs. This apparatus has greatly increased the electromotion and some of its effects; but the phenomena are

too

too great and complicated to be so distinctly analysed as in the original pile.

Excitement of galvanic electricity, and phenomena of the electric column.

Returning to the column—a certain degree of moisture, however, is necessary to the electromotion, in order to give a sufficient conducting faculty to the pieces of paper between the pairs of metals. This I have found by experience; for, having dried every part of a column, placing them near the fire of my chimney when I remounted it in that state, the electromotion had almost ceased in it; but having dismounted it, and laid all the separate pieces on a table, where they remained the whole night, acquiring thus the degree of moisture of the air in the room, when it was made up again, and the same degree of electromotion was restored.

After having demonstrated, and I think without possibility of doubt, from the electromotion in the electric column, and from the phenomena of the galvanic pile itself, that in the latter that motion does not depend on any chemical agency, I come to explain the great improvements made by Mr. Singer in my original apparatus. This explanation, Sir, is the more necessary for your readers, since I could not find myself, in his description of his column, the causes of the great increase of its effect; especially as he mentions only one, of two essential changes he has made, and of this I shall first treat.

This change is expressed in a few words of the following passage. "The combination which has been here employed, consists in two disks of paper interposed between each pair of metals, one disk being pasted on the silver, and the other disk unconnected with either metals." The great improvement which I am now considering, is that disk of paper unconnected with either metals; and surely none but those who have often mounted columns, can understand the effect of that circumstance without an explanation.

This apparatus is unavoidably much handled in its construction, because the zinc and the coppered, or silvered paper disks are to be set together so as to form on the outside a smooth cylinder. But by this operation many paper disks are brought outwards into contact with one another; and wherever

this

Excitement of galvanic electricity, and phenomena of the electric column.

this happens, they destroy the effect of each other. I was aware of that effect, and endeavoured to prevent it as much as I could.

I understood, therefore, the benefit resulting from that unconnected disk of paper, as producing a distance between the metallic papers; but I have only found its great effect by making use of it. I have placed a piece of thick paper between the pairs of one of my columns: while I mounted it I saw that these additional pieces prevented the mutual contact of the pieces of coppered paper; and when it was mounted, I was astonished to find that the electromotion had more than doubled in that column. Such is one of the great improvements made by Mr. Singer in the electric column, which he is so modest as not to point out himself.

I come to the second improvement, no less important, made by the same distinguished experimental philosopher, in the electric column, which has also much increased its effect: it is the more necessary to mention it, that he does not speak of it himself; and it therefore is known only to those who have attended his lectures; besides, had he only mentioned it in his paper, the mere description would not have conveyed the idea of its effect.

Mr. Singer has divided his column into parts of 100 pairs each, inclosed in glass tubes having brass caps fixed with sealing wax at their extremities: brass screws, having a ring outside, pass through the centre of the caps, and serve, both to compress sufficiently the pairs of disks in the tubes, and to produce the communication between those parts of the column, as I had done in connecting together my chaplets.

Such is the other improvement made by Mr. Singer in the column; but, as I have said above, had he only explained it as a part of the construction of his apparatus, the increase of electromotion which it produces had probably been understood by very few of your readers. The cause of that increase is, in itself, a very interesting object of natural philosophy, as it is connected with some important physical phenomena; but for

for its explanation I must recur first, also, to some of my papers in your Journal.

Excitement of galvanic electricity, and phenomena of the electric column.

In your number for October, 1810, I explained the function of the electric column as an aerial electroscope. This last property, which was manifested by the motion of a pendulum, I did not immediately discover, considering only that pendulum in the view that I had intended it at first, to produce the transmission of the electric fluid from one extremity to the other of that apparatus. The columns which I use for that purpose, are terminated at both their extremities by a large brass ball: these balls being brought to a small distance from each other, the pendulum is suspended between them, in communication with the positive extremity of the column: now, its motion points out, almost to the eye, the transmission of the electric fluid from the positive to the negative extremity in that interrupted circuit. When the pendulum returns to the positive ball, it soon recedes from it, is then carried to the negative ball, and so on alternately; giving thus the idea of a bucket, taking water in one pool, and pouring it into another. That this process is a real transmission of electric fluid from one of the extremities to the other, is proved by the constant alternating motions of gold-leaf electroscopes, when connected with these extremities.

By observing some time this interesting phenomenon of the alternate strikings of the pendulum, I come to take notice, that they differed in frequency, not only from one day to another, but in different parts of the same day; and I could not find any connection of these changes with either heat or moisture, which had very little variations in my room, as can be seen from the tables of my observations, with all their circumstances, in the same number of your Journal.

Such is the phenomenon which I attributed to the changes of the electric state of the ambient air, being led to this conclusion by the discovery of Volta on the cause of the motions of the balls in our electroscopes, which discovery I consider as a very important era in the electric science, as are many others of that justly-celebrated experimental philosopher; for to him, in

Excitement of galvanic electricity, and phenomena of the electric column.

in particular, is due the consolidation of Dr. Franklin's theory of the positive and negative electricities, in a manner which is first to be explained.

That theory, though true in itself, encountered at first strong oppositions, because it was not well determined. Positive and negative states of every kind suppose an intermediate or neutral point. Dr. Franklin, therefore, fixed one; considering the neutral point between positive and negative electricities, as being the electric state of the ground, which state he supposed to be both natural and invariable; and as this position was supposed also necessary to the theory of plus and minus, it was attacked under different forms; but especially Dr. Peart, considering that theory as applied to the motions of electroscopes in various cases, compared to a supposed invariable electric state of the ground, made against it such objections as were never satisfactorily answered.

But Volta struck at the root of this dispute, by fixing the true neutral point with respect to the motions of the balls in the electroscopes, which discovery spread a great light on the atmospheric phenomena. He placed that neutral point in the actual electric state of the air in which the balls stood, a state which was variable, so that a pair of balls remaining themselves in the same electric state, diverge differently when transported into a place, the air of which is in a different electric state than the former. This being a fact which I have ascertained by various experiments, could not but point out the influence of the actual electric state of the air on those motions, which was to be explained.

Volta derived that explanation from a true knowledge of the influence of the surrounding air on those motions; he proved that air itself possesses the electric fluid as well as the bodies which it surrounds; that it tends to share it equally with them, imparting some to those which have proportionally less, and taking some from those which have proportionally more than itself.

That discovery also put an end to all the complicated theories by which it had been attempted to explain the phenomenon, that

a pair

Excitement of galvanic electricity, and phenomena of the electric column.

a pair of balls diverge as well when negative as when positive, on which the then prevalent theory of Æpinus introduced the most uncouth association of hypotheses. Volta put also a stop to these fanciful theories, by proving, that it is not by repulsion, that the balls of the electroscopes recede from each other, either positively or negatively; but by each of them being carried towards the air on the outside of them, the electric state of which differed more from their own than that of the air between them, as they both contributed to bring it to their electric state. This solution, both simple and founded on direct experiments, has removed all the difficulties which the motions of the electroscopic balls had presented at first to electricians.

It is from that theory which Sig. Volta had explained to me, with many other particulars in our conversation, that I soon understood the phenomenon of the electric column, which made me add to its first appellation that of aërial electroscope. The surrounding air acts constantly to lessen the difference which naturally exists between the electric states of the extremities of the column; taking some electric fluid from the positive, and imparting some to the negative, but more or less according to its own electric state, which, by its phenomena as an aërial electroscope, is found to be variable; and this circumstance would afford a new proof (if any were wanting) that the difference which exists between the extremities of the column, consists in the quantity of electric fluid.

This preliminary statement was necessary to explain the cause of the great increase of power in Mr. Singer's column, produced by inclosing each 1000 pairs of plates in a glass tube: and, in this respect again, though having that previous knowledge, I should not have conceived how far this increase extended, had not Mr. Singer been so good as to send me one of these parts of 1000 pairs of his column, the power of which, compared to the same number of my column, has astonished me; but I soon conceived the cause of that difference, which I now can explain.

The air confined in these glass tubes, has very little commu-

Excitement of galvanic electricity, and phenomena of the electric column.

nication with the outside air, and has not a sensible agitation in that inclosed space. Thus, the difference of the electric states, produced by the action of the column at its extremities, is almost entirely preserved. Now, the great increase in the difference between the positive and negative states at these extremities, resulting only from mere preservation, manifests a much greater electromotion produced by the simple association of zinc and silver plates in pairs separated by paper, than appeared in the column as I had first constructed it.

After this statement of the phenomena of the electric column, there remains very little more for me to add concerning the second object announced in the title of this paper, which is, to compare these phenomena with the theories of the excitement of galvanic electricity collected by Dr. William Henry in his paper published in your number for August.

The electromotion in the galvanic pile is a common object in all these theories; and in all of them it is attributed, though in various manners, to chemical agency. Mr. Henry is very fair in his judgment of the theories which he successively details; stating the objections which he finds himself against them, though setting out from the principles of their respective authors. Had he, therefore, been informed of the phenomena of Mr. Singer's column, which produces more electromotion than any galvanic pile which has ever existed, without any appearance of chemical agency, either between its extremities, or in itself, he would have found the solution of all the difficulties which he has himself pointed out in these theories, being thus informed, that no connection exists between the electromotion and chemical effects in the galvanic pile.

This interesting question, Sir, will surely be brought to an issue, if you admit this paper in your valuable Journal; for I cannot doubt, that those experimental philosophers who shall not be satisfied with my arguments on this very interesting point of natural philosophy, will undertake experiments to discover my errors; and, on whatever side the decision shall be final, it will be a fixed step in that science, which will give

me

me satisfaction, as the discovery of real causes, not imaginary, in the physical phenomena, is my only object.

I have the honour to be,

Sir,

Your most obedient Servant,

J. A. DE LUC.

*Windsor, Sep.* 14, 1813.

---

IV.

*Observations on the Celestial Bodies, made in the day-time; particularly on the Planet Venus, with some new Deductions in relation to that Planet. In a letter from Mr.* T. DICK.

THE following communication contains specimens and results of several hundreds of observations, made with a view to determine the following particulars.

Particulars to be ascertained by day observations.

1. What stars and planets may be conveniently seen in the day-time, when the sun is above the horizon?

2. What degrees of magnifying power are requisite for distinguishing them?

3. How near their conjunction with the sun they may be seen.

4. Whether the diminution of the aperture of the object glass of the telescope, or the increase of magnifying power, conduces most to render a star or a planet visible in day-light.

Observations, similar to some of those here stated, have, doubtless, been made by astronomers, and may probably be recorded in certain Scientific Journals; but as the writer of this had never seen any of them in those Journals he has had access to peruse, and as they are not to be found in the largest systems of astronomy he has consulted, he was induced to make them for his own satisfaction and amusement, and they are now communicated for the information of those who may not be disposed to undertake the same labour, and who have no access to other sources of information. Some of the observations, in relation to Venus, it is presumed are original.

The instrument described.

The observations were made by means of a small *equatorial instrument*, surmounted with a telescope made by W. and S. Jones,

Telescope made use of.

Jones, Holborn, London. The telescope, which originally accompanied the instrument, was an achromatic refractor, its object glass being $8\frac{1}{2}$ inches focal distance, and one inch diameter, carrying a magnifying power of seven times. It was afterwards furnished with an eye-piece, which produced a magnifying power of 45 times, which it bore with great distinctness. With this telescope and the power last mentioned, several of the observations were made. But the greater number were made with an achromatic telescope, having its object glass 20 inches focus, and about $1\frac{3}{4}$ inch diameter, which was afterwards attached to the equatorial machinery in place of the small telescope above-mentioned. It was furnished with magnifying powers of 15, 30, 45, 60, and 100 times. The instrument was fixed in a position nearly fronting the south; but could occasionally be removed to another position fronting the north-west.

The observations were began about the end of August, 1812, and were continued, as opportunity offered, to the same period in 1813. The latitude of the place of observation is about 56° 30′ north. The particular observations stated, are taken from memorandums noted at the time of observation.

*Observations on fixed Stars of the first Magnitude.*

Observations upon fixed stars.

April 23d, 1813. 10h. 15′ A. M. the sun being $5\frac{1}{2}$ hours above the horizon. Saw the star Vega, or *a Lyræ*, very distinctly with a power of 30 times. Having contracted the aperture of the object glass to nine-tenths of an inch, saw it on a darker ground, but not more plainly than before. Having contracted the aperture still farther, to half an inch, I perceived the star, but not so distinctly as before. The sky being very clear, and the star in a quarter of the heavens nearly opposite to the sun, I diminished the magnifying power to 15, and could still perceive the star, but indistinctly; it was just perceptible. April 22d. Saw Aldebaran at 1h. 40′ P. M. very distinctly with a power of 60; the aperture not diminished. Same day, at 2h. 15′ P. M. saw Betelgeux, or *a Orionis*, with a power of 60, and immediately afterwards with a power of 30; the

Observations upon fixed stars in the day-time.

the aperture in both cases undiminished. It appeared brighter than Aldebaran. August 23d. 0h. 12′ P. M. saw the star Capella or α *Aurigæ,* with a power of 60, and immediately afterwards with a power of 30; the aperture undiminished. With this last power it appeared extremely distinct, but not so brilliant and splendid as with the former power. Having diminished the aperture to nine-tenths of an inch, it appeared on a darker ground, though, in the former case, it was equally perceptible. A few minutes afterwards, could distinguish it with a power of 15; the aperture being contracted to half an inch. It appeared very small; it was with difficulty the eye could fix upon it in the field of the telescope; but when it was once perceived, its motion across the field of view could be easily followed. Could not perceive it when the diminished aperture was taken away.

August 10. 9h. 30′ A. M. Saw the star Sirius, with a power of 60, the aperture contracted to nine-tenths of an inch. Saw it likewise when the aperture was diminished to half an inch, but not nearly so distinct. Saw it also when the aperture remained undiminished, but not so distinctly as through the aperture of nine-tenths of an inch. Having put on a power of 30, could distinguish it distinctly enough through each of the former apertures, and also when they were removed, but somewhat more distinctly with the apertures of nine-tenths and half an inch, than without them. At this time the star was 2h. 42′ in time of right ascension west of the sun, having an elevation above the horizon of about 17° 10′; the sun shining bright, and the sky very much enlightened in that quarter of the heavens where the star appeared. There was also a considerable undulation in the air, which is generally the case in the hot mornings of summer, which renders a star more difficult to be perceived than in the afternoon, especially when it is viewed at a low altitude. June 4th. 1h. 30′. Saw Sirius, with a power of 30 with great distinctness; the aperture not contracted. The star was then 1h. 50′ in time of right ascension east of the sun. August 24th. 9h. 5′ A. M. Saw the star Procyon, or α *Canis Minoris,* distinctly with a power of 60, the aperture

not

Observations upon fixed stars in the day-time.

not diminished. When diminished to nine-tenths, it appeared rather more distinct, as the ground on which it was seen was darker. With a power of 30, and the aperture contracted to nine-tenths of an inch, could perceive it but somewhat indistinctly. When the equatorial motion was performed in order to keep it in the field of view, it was some time before the eye could again fix upon it. When the aperture was diminished to half an inch, it could not be perceived. Saw it when both the apertures were removed, but rather more distinctly with the aperture of nine-tenths of an inch. The difference in the result of this observation, from that of Capella, above stated, was owing to the star's proximity to the sun, and the consequent illumination of the sky in that quarter where it appeared. Its difference in right ascension from that of the sun was then about 2h. 50′ in time, and its difference of declination about 4° 50′*. Procyon is marked by Mackay, in his "Complete Navigator," as a star between the first and second magnitudes.

June 3. Observed Arcturus, or *a Bootis*, very distinctly a little before seven in the evening, the sun being about 1h. 40′ above the horizon, and shining bright, with a power of 15; the aperture not contracted. It appeared very small, but distinct. This star is easily distinguishable at any time of the day with a power of 30.

Similar observations to the above were made, and frequently repeated, on the stars Rigel, Cor Leonis, and other stars of the first magnitude, which gave nearly the same results. The stars Altares and Formalhaut are not so easily distinguished, on account of their great southern declination, and consequent low elevation above the horizon.

* The right ascensions, declinations, longitudes, &c. stated in this paper, are only approximations to the truth; perfect accuracy in these respects being of no importance in these observations. They are, however, in general, within a minute or two of the truth. The times of the observations, too, are noted, in reference, not to the *astronomical*, but to the *civil* day.

Obser-

### *Observations on Stars of the second Magnitude.*

My observations on stars of the second magnitude have not been so numerous as those on stars of the first magnitude. The following are extracts of some of those which were made.

Observations upon fixed stars in the day-time.

November 12th, 1812, 1h. 30′ P. M. Saw the star Altair, or *a Aquilæ*, with the eight-inch telescope, carrying a power of 45, the aperture not contracted. Having contracted the aperture a little, it appeared somewhat less distinct. This star is reckoned, by some, to belong to the class of stars of the first magnitude. In White's Ephemeris it is marked as being of the 2d magnitude. It is obviously less splendid than a Lyræ or a Bootis, and more brilliant than *a Ophiuchi*.—May 5, 1813, 6h. P. M. The sun being about an hour and three quarters above the horizon. Saw Alphard, or *a Hydræ*, a star of the 2d magnitude, with a power of 60, the aperture diminished to 9-10ths of an inch; a few minutes afterwards could perceive it, but indistinctly, with a power of 30, the aperture contracted as above. It could not be seen very distinctly with this power till about half an hour before sun-set. Was then seen rather more distinctly when the aperture was contracted than without the contraction.—May 7. Saw the star Deneb, or *C Leonis*, distinctly with a power of 60, about an hour and a half before sun-set.—August 20th. Saw Ras Alkague, or *a Ophiuchi*, at 4h. 40′ P. M. with a power of 100; the sun being nearly 3 hours above the horizon and shining bright. Perceived it about an hour after with a power of 60, with the aperture contracted to 9-10ths, and also when this contraction was removed. The star was seen nearly as distinctly in the last case as in the first. August 27. 5h. P. M. The same star appeared quite distinct with a power of 60; the aperture not contracted. Did not appear more distinct when the aperture was contracted to 9-10ths of an inch. The sun was then more than 2 hours above the horizon.—August 28th, 7h. A. M. Saw the star Pollux, or *C Gemini*, 2 hours after sun-rise, with a power of 60; aperture undiminished.

Similar observations, giving the same results, were made on the

Observations upon fixed stars in the day-time.

the stars Bellatrix, Orion's Girdle, a Andromedæ, a Pegasi, Aleoth, Benetriach, North brown, and several other stars of the 2d magnitude.

All the above stated observations, as well as those which follow, were made with the 20-inch telescope (unless otherwise stated) and in every instance the sun was shining bright.

From the above, and similar observations made on the fixed stars, the following conclusions are deduced.

1. That a magnifying power of 30 times is sufficient for distinguishing a fixed star of the 1st magnitude, even at noon day, at any season of the year; provided it have a moderate degree of elevation above the horizon, and be not within 30° or 40° of the sun's body. Also, that, by a magnifying power of 15, a star of this class may be distinguished, when the sun is not above an hour and a half above the horizon. But in every case higher powers are to be preferred. Powers of 45 and 60, particularly the last, were found to answer best in most cases, as with such powers the eye could fix on the star with ease as soon as it entered the field of the telescope.

2. That most of the stars of the 2d magnitude may be seen with a power of 60 when the sun is not much more than 2 hours above the horizon; and at any time of the day, the brightest stars of this class may be seen with a power of 100 when the sky is serene, and the star not too near the quarter in which the sun appears.

3. That in every instance, an increase of magnifying power, has the principal effect in rendering a star easily perceptible.—That diminution of aperture, in most cases, produces a very slight effect, in some cases none at all, and when the aperture is contracted beyond a certain limit, it produces a hurtful effect. The cases in which a moderate contraction is useful, are the two following: 1. When the star appears in a bright part of the sky, not far from that quarter in which the sun appears. 2. When an object glass of a large aperture, and a small degree of magnifying power are used. In almost every instance the contraction of the object glass of the 8-inch telescope, with a power of 45, had a hurtful effect. But when the 20-inch telescope

carried

carried a power of only 15, the contraction served to render the object more perceptible.

## *Observations on the Planet Venus.*

My observations on Venus commenced about the end of August, 1812, about 3 or 4 weeks after her inferior conjunction. About that time, between ten and eleven in the forenoon, with a power of 45, she appeared as a beautiful crescent, quite distinct and well-defined, with a lustre similar to that of the moon about sun-set, but of a whiter colour. The view of her surface and phase was almost as distinct and satisfactory, as what is obtained in the evening after sun-set. She was then very distinctly seen with a power of 7 times. I traced the variation of her phases, almost every clear day, till the month of May, 1813. As at that time she was not far from her superior conjunction with the sun, I wished to ascertain, how near her conjunction with that luminary she might be seen; and particularly whether it might not be possible, in certain cases, to see her at the moment of her conjunction. Observations upon the planet Venus in the day time.

The uniform language of astronomical writers on this point, gave me no reason to believe that my observations would be crowned with success. Their expressions, when describing the phases of Venus, either directly assert, or at least imply, that it is impossible, in any instance, to see that planet at the time of her superior conjunction.—Mr. Benjamin Martin, in his "Gentleman and Lady's Philosophy," vol. 1. p. 54. says, "At and about her upper conjunction she cannot be seen, by reason of her nearness to the sun." That this was not an inadvertent expression, appears, from the same idea being repeated in different words, when treating the subject in a more scientific manner, in his *Philosophia Britannica,* vol. 3. p. 135. "At her superior conjunction Venus would appear a full enlightened hemisphere, *were it not that she is then lost in the sun's blaze,* or hid behind his body." Dr. Long, in his Astronomy, vol. 1. book 2. ch. 11. says, "Venus, at her superior conjunction, *if she could be seen,* would appear round like the full moon."

A late

Observations upon the planet Venus in the day time.

A late writer on Astronomy, whose diction is more original than several other popular writers on that subject, repeats the following proposition in a different form three different times, "when Venus is on the other side of the sun, in which she appears in the same sign with him, *she cannot be seen in the heavens*, being lost in the refulgence of the solar rays," &c. *Bryan's Astronomy*, 3d edition, pp. 238. 244. Dr. Brewster, the latest and most respectable writer I have had access to consult, when describing the phases of Mercury and Venus, says, "their luminous side is completely turned to the earth at the time of their superior conjunction, when they would appear like the full moon, *if they were not then eclipsed by the rays of the sun**." The same opinion is expressed in similar phrases by Ferguson, in his "Astronomy explained," &c. Adams, in his "Astronomical Essays," G. Gregory, in his "Lectures," and by every other writer on the subject to whose works I have had access. How far such expressions are correct will appear from the following observations and remarks.

April 24, 1813, 10h. 50′. A. M. Observed Venus with a power of 30; the aperture not contracted. She was then about 31′ in time of right ascension distant from the sun. Their difference of declination 3°. 59′. She appeared distinct and well defined. With a power of 100 could distinguish her gibbous phase.—May 1, 10h, 20′. A. M. Viewed Venus with a power of 60; the aperture, not contracted. She appeared very distinct. Saw her at the same time with a power of 15; the aperture being contracted to 9-10ths of an inch. Having contracted the aperture to ½ inch, saw her more distinctly. When the contracted apertures were removed, she could with difficulty be distinguished, on account of the direct rays of the sun striking on the inside of the tube of the telescope. The sun was shining bright, and the planet about 25′ in time of R. A. were of his centre; their difference of declination being 3°. 7′.—May 7, 10h. A. M. Saw Venus distinctly with a power of 60; the sun shining bright. She was then about 19′ in time of R. A.

* Edinburgh Encyclopædia, Art. *Astronomy*, p. 655.

and

and 4°. 47′ in Longitude west of the sun. Their difference of declination being 2°. 18′. I found a diminution of aperture particularly useful, when viewing the planet at the time, even when the higher powers were with the sun, which happened on May 25, at 9h. 30′. A. M. Her geocentric latitude at that time, being about 16′ S. She must have passed almost close by the sun's southern limb. Cloudy weather for nearly a month after the last observation, uniformly prevented any further observations, when the planet was in that part of the heavens which was within the range of the instrument. The first day which proved favourable for observation after she had passed her conjunction was the 5th of June. The following is the memorandum of the observation then taken.

Observations upon the planet Venus in the day-time.

June 5th, 9h. A. M. Adjusted the equatorial telescope for viewing the planet Venus, but she could not be perceived on account of the direct rays of the sun entering into the tube of the telescope. I contrived an apparatus for screening his rays, but could not get it conveniently to move along with the telescope; and, therefore, determined to wait till past eleven, when the top of the window of the place of observation would intercept the solar rays. At 11h. 20′. A. M. Just as the sun had passed the line of sight from the eye to the top of the window, and his body was eclipsed by it, I was gratified with a tolerable distinct view of the planet with a power of 60; the aperture being contracted to 9-10ths of an inch. The distinctness increased as the sun retired, till, in two or three minutes she appeared perfectly well defined. Saw her immediately afterwards, with a power of 30; the aperture contracted as before. Saw her also quite distinct with a power of 15, but she could not be perceived with this power when the contracted aperture was removed. At this time Venus was just 3° in longitude, or about 13′ in time of R. A. east of the sun's centre, and of course, only about $2\frac{1}{2}$° from his eastern limb. The difference of their declination being about 27′, and the planet's latitude about 11″ north.

As I considered this observation of some importance in determining the object I had in view; and as it might possibly,

by

Observations upon the planet Venus in the day-time.

by some, be called in question; about two days after, I called in the Reverend J. Jameson, of Methuen, a gentleman of literary acquirements, to whom I shewed Venus in the manner stated above, at 11h. 23′ A M. and, although he is not so much accustomed, as the writer of this, to look through telescopes, he perceived the planet *very distinctly* with a power of 15, (a diagonal eye-piece being used) having also seen it immediately before with a *direct* eye-piece carrying a power of 60.

June 7. 10h. Saw Venus with a power of 60, the aperture being diminished to 9-10ths, the direct rays of the sun *not being intercepted by the top of the window*. The aperture having been further contracted to $\frac{1}{2}$ inch, could perceive her, but not quite so distinctly. When the contractions were taken off, she could scarcely be seen. She was then 3° 33′ in longitude, and nearly 15′ in time of R. A. distant from the sun's centre. Some fleeces of clouds having moved across the field of view, she was seen remarkably distinct in the interstices; the sun at the same time being partly observed by them.—June 24th. 10h. A. M. Observed Venus with powers of 100, 60, and 15. With the power of 15, aperture 9-10ths of an inch, saw her distinctly. Having contracted the aperture to half an inch, saw her more distinctly than with the contraction of 9-tenths. She was then about 35′ in time of R. A. east of the sun.—August 19th. 1h. 10′, P. M. Viewed Venus with a magnifying power of 100. Could perceive her surface and gibbous phase, almost as distinctly as when the sun is below the horizon. She appeared bright, steady in her light, and well-defined, without that glare and tremulous appearance she exhibits in the evening when near the horizon. She was then nearly on the meridian. On the whole such a view of Venus is as satisfactory, if not preferable to those views we obtain with an ordinary telescope, in the evening, when she is visible to the naked eye*.

The

* The late Mr. B. Martin, when describing the nature and effects of the solar telescope in his *Philosophia Britannica*, vol. 3. p. 85. gives the following relation. " I cannot here omit to mention a very *unusual phenomenon* that I observed about ten years ago in my darkened room. The window looked towards the west, and the spire of Chichester Cathedral was directly before it at the distance of about 50 or 60 yards. I used

The following conclusions are deduced from the observations made on Venus.

Observations upon the planet Venus in the day-time.

1. That Venus may be seen distinctly, with a moderate degree of

I used very often to divert myself by observing the pleasant manner in which the sun passed behind the spire, and was eclipsed by it for some time; for the image of the spire and sun were very large, being made by a lens of 12 feet focal distance. And once as I observed the occultation of the sun behind the spire, just as the disk disappeared, I saw several small, bright, round bodies or balls running towards the sun from the dark part of the room, even to the distance of 20 inches. I observed their motion was a little irregular, but rectilinear, and seemed accelerated as they approached the sun. These luminous globules appeared also on the other side of the spire, and preceded the sun, running out into the dark room, sometimes more, sometimes less, together in the same manner as they followed the sun at its occultation. They appeared to be in general 1-20th of an inch in diameter, and therefore must be very large luminous globes in some part of the heavens, whose light was extinguished by that of the sun, so that they appeared not in open day light; but whether of the meteor-kind, or what sort of bodies they might be, I could not conjecture."—I was always at a loss, till lately, to conceive what kind of bodies the luminous globes here mentioned might be. But in my frequent views of Venus, when very near the sun, I several times observed a similar phenomenon. Sometimes one, and sometimes 4 or 5 round luminous bodies, some larger and some smaller than the apparent size of the planet, moved across the field of the telescope in a rectilinear direction. When the equatorial motion was performed in order to search for the planet, or to keep it in the field, I once or twice mistook one of these bodies for the planet, till its rapid motion undeceived me. I soon, however, ascertained, beyond a doubt, that these bodies were nothing more than birds of different sizes, and at different distances; the convex surface of whose bodies was in such a position as strongly to reflect to the eye the solar rays; their true figure being undistinguishable by reason of their motion and their distance. When they were near and appeared larger, their shape could be in some measure distinguished. Sometimes small winged insects at no great distance from the telescope presented a similar appearance. I seldom or never observed the phenomenon now described, except when the telescope was pointed in a direction nearly to that of the sun. There can be no doubt that this was the cause of the phenomenon observed by Mr. Martin; and I state this circumstance, to shew, with what caution we should draw conclusions

Observations upon the planet Venus in the day-time.

of magnifying power, *at the moment of her superior conjunction with the sun,* when her geocentric latitude, either north or south, at the time of conjunction, is not less than 3 degrees. This conclusion is deduced from the observation of June 5th; for if Venus may be seen when only 3° east of the sun, it is evident, and requires no illustration, that she may be equally well seen at the same distance, either north or south of the sun's centre. Nay, the latter are better positions than the former for such an observation; as the direct solar rays can be more easily and conveniently intercepted when Venus is north or south of the sun, than when she is on the east or the west of him. I have stated 3° as the limit beyond which she may be seen; because I have not had occular demonstration that she may be seen within this limit; but I am at the same time of opinion, from the degree of distinctness with which she appeared at the time referred to, that she may be perceived when she is distant only $1\frac{1}{2}$° from the centre of the sun; provided his rays be properly intercepted.—In order to view this planet to advantage at any future conjunction, when in south latitude, it will be proper to fix a board at a small distance beyond the object end of the telescope, having such a degree of concave curvature, as shall nearly correspond with a segment of the diurnal arc, at that time described by the sun; with its lower concave edge at an elevation a small degree above the line of collimation of the telescope when adjusted for viewing the planet; in order completely to intercept the solar rays. When the planet is in north latitude the curvature of the board must be made convex, and placed a little below the line of sight. The opposite figures will illustrate my idea; where A B (fig. 1) represents the concave curve of the board to be used when the planet is in south

clusions respecting any unusual phenomenon, which we have but once observed, and had no time to examine; since the ingenious mathematician and philosopher now mentioned; seems to have concluded, that, what was in reality a flight of birds, were "large luminous globes in the heavens."—This note, suggested by the subject of this paper, though not necessarily connected with it, it is hoped will be excused.

latitude;

Observations upon the planet Venus in the day-time.

latitude; $c\ d$ a segment of the apparent diurnal path of the planet; and $e\ f$ a segment of the sun's diurnal arc. Fig. 2. represents the board to be used when the planet is in north latitude, which needs no farther description*.—By this means Venus may be viewed with ease, for several hours about the time of her conjunction, and with considerable distinctness, if her latitude exceeds 3°, and her elevation above the horizon be considerable.

The circumstance now ascertained, that Venus may be seen at the time of her superior conjunction, may not be considered as a fact of much importance in astronomy. It is always useful, however, in every department of science, to ascertain every fact connected with its principles, however circumstantial and minute; as it tends to give precision to its language, and may ultimately promote its progress, by leading to conclusions which were not at first apprehended. The present fact may possibly lead to the determination of the difference (if any) between the polar and equatorial diameters of Venus, which has hitherto remained undetermined. It is well known that the earth is of a spheroidal figure, having its polar shorter than its equatorial diameter. Jupiter has long been known to exhibit a similar figure, which is perceptible by a telescope of moderate power; and Dr. Herschel has lately found, that Mars and Saturn are also oblate spheroids, and has determined the proportion between their equatorial and polar diameters. As Venus revolves about her axis with a swiftness as great as that of the earth, it is reasonable to conclude that she is of a similar figure. It is difficult, however, if not impossible, to determine this point by observation, when she is in those positions in which she has most frequently been viewed; as at such times she assumes either a gibbous phase, or the form of a half-moon, or that of a crescent. I am, therefore, of opinion, that, at some future conjunction, when her geocentric latitude is considerable, with a telescope of a high magnifying power, furnished with a micrometer, this point might be ascertained. If she is then

* In this description the observer is supposed to be in north latitude.

Observations upon the planet Venus in the day-time.

viewed at a high altitude, and the sky serene, her disc will appear sufficiently luminous and well defined for this purpose, free of that glare, and tremulous aspect she generally exhibits in the evening, when near the horizon; which makes her appear larger than she ought to do, and prevents her margin from being accurately distinguished.

2. Another conclusion from the observations on Venus is:—That during the space of 583 days, or about 19 months, the time she takes in moving from one conjunction with the sun to a like conjunction again, when her latitude at the time of her superior conjunction exceeds 3°,—she may be seen with an equatorial telescope, every clear day, without interruption; except at the time of her inferior conjunction, when her dark side is turned towards the earth, and 3 or 4 days before and after it. When her latitude is less than 3° she will be hid only 11 days before, and the same time after her superior conjunction. During the same period she will be invisible to the naked eye, and consequently no observations can be made on her with a common telescope, for nearly 8 months, and sometimes more, according as her declination is north or south; viz. above three months before, and the same time after her superior conjunction, and above 2 weeks before and after her inferior conjunction; except where there is a very free and unconfined horizon.—It follows,

3. That every variation of the phases of this planet, from a slender crescent to a full enlightened hemisphere, may, on every clear day, be conveniently exhibited. This circumstance I have found extremely useful, when explaining the appearances of the inferior planets to young persons, and endeavouring to convince them of the truth of the solar system. The phases of the inferior planets, considered in reference to their situation with respect to the sun, is, perhaps, the best popular argument for convincing a tyro of the truth of the Copernican system.—Having placed Venus in her true position on an orrery, by means of an ephemeris, I desire the pupil to mark her phase, as seen from the earth, which is also placed in its true position. I then adjust the equatorial telescope for Venus, and

shew

shew him the planet with the same phase in the heavens. This exhibition, however, can seldom be made, if we must wait till the planet be visible to the naked eye, which we must do, if not furnished with an equatorial instrument. As actual observations on the planets in the heavens make a deeper and more convincing impression on the mind of a tyro, than mere diagrams, or verbal explanations, I consider an equatorial telescope, in conjunction with a celestial globe, and an orrery, as essentially necessary to every private teacher of astronomy, as, independent of its use, now hinted at, it is the best instrument for conveying an idea of the practical operations of that science.

Observations upon the planet Venus in the day-time.

4. I am of opinion, that useful observations on the surface of Venus might sometimes be made in the day-time, with telescopes of great magnifying power, when I consider the degree of brilliancy she exhibits even in day-light. Such observations might, perhaps, for ever set at rest those disputes which have arisen respecting the time of the rotation of this planet. Cassini, from observations on a bright spot, which advanced 20° in 24h. 34′, determined the time of her rotation to be 23h. 20′. On the other hand, Bianchini, from similar observations, concluded, that her diurnal period was 24 days and 8 hours. The difficulty of deciding between these two opinions, arises from the short time in which observations can be made on this planet, either before sun-rise, or after sun-set, which prevents us from tracing, with accuracy, the progressive motion of its spots, for a sufficient length of time. And, although an observer should mark the position of the spots, at the same hour on two succeeding evenings, and find they had moved forward about 20° in 24 hours, he would still be at a loss to determine whether they had moved only 20° in all since the preceding observation, or had finished a revolution, and 20° more. If, therefore, any spots could be perceived on the surface of Venus, in the day-time, their motion might be traced, when she is in north-declination, for 12 hours or more, which would completely settle the period of rotation. That it is not improbable that spots may be discovered on her disk, in the

 day-time,

Observations upon the planet Venus in the day-time.

day-time, appears from some of the observations of Cassini, who saw one of her spots when the sun was more than eight degrees above the horizon*. Nor do I consider it altogether impossible that her satellite (if she have one, as some have supposed) may be detected in the day-time, when she is in a favourable position for such an observation; particularly when a large portion of her enlightened surface is turned towards the earth, and when her satellite, of course, must present a similar phase. If this supposed satellite be about one-third or one-fourth of the diameter of its primary, as Cassini, Short, and Montaigne, supposed, it must be nearly as large as Mercury, which has been frequently seen in day-light. If such a satellite has a real existence, and yet undistinguishable in day-light, its surface must be of a very different quality for reflecting the rays of light, from that of its primary; for it is obvious to every one who has seen Venus with a high power, in the day-time, that a body of equal brilliancy, though four times less in diameter, would be quite perceptible, and exhibit a visible disk. Such observations, however, would be made with a greater effect in Italy, and other southern countries, where the sky is more clear and serene, and where the planet may be viewed in higher altitudes than in this island.

5. Another conclusion, from the observations on Venus, is, that a moderate diminution of aperture is useful, and even necessary, in viewing this planet when near the sun. Its effect is owing, in part, to the direct solar rays being thereby more effectually excluded.

In fine, we may fairly conclude, that such expressions of astronomical writers, when describing the phases of Venus, as those quoted above, are but partially true, and therefore ought either to be laid aside, or qualified, if they would not run the risk of conveying an erroneous idea. The vague and incorrect expressions and statements not only in this, but in several other respects, which abound in several late popular writings on astro-

* See Long's Astronomy, vol. 2, p. 437, or Encyclopædia Britannica, vol. 2 p, 436. 3d edition.

nomy,

nomy, and which have been copied by one writer from another, without examination, tend to throw disgrace on a science, whose leading facts and principles have been so accurately established.

Observations upon the planet Venus in the day-time.

*Observations on Jupiter.*

This planet is very easily distinguished in the day-time, with a very moderate magnifying power, when he is not within 30° or 40° of the sun. The following extract from my memorandums may serve as a specimen. August 28th, 1813. 1h. 40′ P. M. saw Jupiter with a power of 15, the aperture not contracted. He appeared so distinct with this power, that I have reason to believe, he would have been perceived with a power of 6 or 7 times. When the aperture was contracted to nine-tenths, and afterwards to half an inch, there was little perceptible difference in his appearance. He was then about 58° in longitude east of the sun.

Though Jupiter, when at, and near his opposition to the sun, appears to the naked eye with a brilliancy nearly equal to that of Venus, yet there is a very striking difference between them in respect of lustre, when viewed in day-light. Jupiter, when viewed with a high magnifying power, in the day-time, always exhibits a very dull, cloudy appearance; whereas Venus appears with a moderate degree of splendour. About the end of June, 1813, between 5 and 6 in the evening, having viewed the planet Venus, then within 20° of the sun, and which appeared with a tolerable moderate degree of lustre, I directed the telescope to Jupiter, at that time more than 32° from the sun, when the contrast between the two planets was very striking, Jupiter appearing so faint as to be but just discernible, though his apparent magnitude was more than double that of Venus. In this observation a magnifying power of 60 was used. In his approach towards the sun, about the end of July, I could not perceive him when he was within 15° or 16° of his conjunction with that luminary. These circumstances furnish a sensible and popular proof, independent of astronomical calculations, that Jupiter is removed at a much greater

Observations upon the planet Venus in the day-time.

greater distance from the sun than Venus; since his light is so faint as to be scarcely perceptible when more than 20° from the sun, while that of Venus is distinctly seen amidst the full splendour of the solar rays. With a power of 60 I have distinguished the belts of Jupiter before sun-set, but could never see any of his satellites till the sun was below the horizon.

My observations on Saturn in day-light have not been frequent. Have seen his ring several times before sun-set with a power of 60; but his great southern declination, and consequent low altitude, for a considerable time past, have been unfavourable for such observations; and therefore no general conclusions can be deduced from them. The southern declination of Mars having of late been greater than even that of Saturn, has also prevented any satisfactory observations on him, when the sun was above the horizon. With respect to Mercury, I have seen him several times after sun-rise, and before sun-set, about 10 or 11 days before and after his greatest elongation, with a power of 45. I once or twice looked for him about mid-day, but did not perceive him. The air, however, at the times alluded to, was not very clear, and I was not perfectly certain that he was within the field of the telescope; and therefore I am not convinced but that, with a moderately high power, he may be seen even at noon-day.

Such, then, are some of the observations I have made on the heavenly bodies, in the day-time, and the conclusions which may be deduced from them. I have been induced to communicate them from the consideration, that the most minute facts, in relation to any science, are worthy of being known, and may possibly be useful. They may, at least, gratify the astronomical tyro with some information which he will not find in the common treatises on astronomy. Besides those already stated, the following general conclusions may be noted. 1. That a celestial body may be as easily distinguished at noon-day as at any time between the hours of nine in the morning and three in the afternoon, except during the short days in winter. 2. They are more easily distinguished at a high than at a low altitude; in the afternoon than in the morning,

ing, especially if their altitudes are low : and in the northern part of the heavens than in the southern. The difficulty of perceiving them, in the first case, is obviously owing to the thick vapours near the horizon : in the second case, to the undulation of the air, which is generally greater in the morning than in the afternoon. This may very evidently be perceived by looking at a distant land object, at those times, in a hot day, through a telescope that magnifies 30 or 40 times: in the third case it is owing to the northern part of the sky being of a deeper azure on account of its being less enlightened than the southern, with the splendour of the solar rays.

Observations upon the planet Venus in the day-time.

Had I not already protracted this communication too far, I intended to have offered a few remarks on the utility of the equatorial telescope, and the practical uses to which observations on stars, in the day-time, may be applied. I shall only at present observe, that in accurately adjusting circular and transit instruments, it is useful, and even necessary, for determining the exact position of the meridian, to take observations of several stars, which differ greatly in zenith distance, and which transit the meridian nearly at the same time. But as the stars best situated for this purpose, cannot, at every season, be seen in the evenings, we must, in certain cases, wait for several months till such observations can be made, unless we make them in the day-time, which can very easily be done, if the instrument has a telescope adapted to it, furnished with such powers as those above stated. I have also freqnently made use of observations on the stars, in the day-time, for adjusting a clook or watch to mean time, when the sun was in a situation beyond the range of the instrument, or obscured by clouds; and when I did not chuse to wait till the evening. This may, at first view, appear to some as paradoxical, since the finding of a star in day-light depends on our knowing its right ascension from the sun, and this last circumstance depends on our knowing the true time. But if a watch or a clock is known not to have varied above seven or eight minutes from the time, a star of the first magnitude may easily be found by moving the telescope a little backwards or forwards, till the star appear ; and when

Observations upon the planet Venus in the day-time.

when it is once found, the exact variation of the movement is then ascertained, by comparing the calculations which were previously necessary, with the time pointed out by the nonius on the equatorial circle. All this may be accomplished in five or six minutes. The equatorial, too, is, perhaps, the best instrument for teaching a learner the names and positions of the principal stars. For when the right ascension and declination of any star is known, the telescope may be immediately adjusted to point to it, which will infallibly prevent his mistaking one star for another. In this way, also, the precise position of the planet Mercury, the Georgium Sidus, a small comet, or any other body not easily distinguished by the naked eye, may be readily pointed out. But to enter minutely into these and other particulars, would protract this paper to an inconvenient length.

In conclusion, I cannot but express my surprise, that the equatorial telescope is so little known, even by many of the lovers of astronomical science. In several respectable academies in this part of Britain, and, if I am not mistaken, in some universities, this instrument is entirely unknown. This is the more unaccountable, as a small equatorial can be purchased for a moderate sum; and as there is no one instrument so well adapted for illustrating all the operations of practical astronomy. Where very great accuracy is not required, it may occasionally be made to serve the general purposes of a transit instrument, a quadrant, an equal altitude instrument, a theodolite, an azimuth instrument, a level, and an accurate universal sun dial. It is to be hoped, the members of the Edinburgh Astronomical Institution, who are just now erecting an observatory, will not not omit an equatorial instrument as part of the apparatus they are now furnishing for astronomical observations.

Should the above communication be acceptable, several other observations and remarks on kindred subjects may afterwards be communicated. Mean time I am your's, &c.

THOMAS DICK.

*Methven, near Perth, N. B.*
*14th Sep.* 1813.

*An*

## V.

*An explanatory Statement of the Notions or Principles upon which the systematic Arrangement is founded, which was adopted as the Basis of an Essay on Chemical Nomenclature. By Professor* J. BERZELIUS, *&c. &c. Received from the Author; in continuation from p. 166 of our XXXVth Volume.*

### IV. *On the Combinations of Tellurium with Oxigen, Hidrogen, and the Saline Bases.*

M. RITTER, some years ago, discovered that tellurium employed as a negative conductor in the operation of the pile, combines with hydrogen, and forms a brownish powder, not having a metallic appearance. Davy afterwards discovered, that tellurium, saturated with hydrogen, forms a peculiar gas, very much resembling sulphuret of hydrogen in its smell and other properties. It follows, from the electro-chemical views we have given in this present treatise, that the principle of acidity is not in oxigen, but that it arises principally from the combustible radicals of bodies. Now, Mr. Davy having found, that the new gas of tellurium and hidrogen (tellurenim hydrogenii) has the property of combining with caustic alkalis, as well as the sulphuret of hydrogen, it follows, of necessity, that the oxide of tellurium must likewise be capable of combining in the form of acid with alkalies and saline bases in general. It was from these considerations that in my essay upon nomenclature I ranged tellurium with the most electro-negative metals, without, however, having an opportunity of examining more correctly the properties of the oxide of this metal. The kindness of Mr. Geyer afterwards afforded me an opportunity to enter into this examination. Mr. Geyer made me a present of three grammes of metallic tellurium, which had formed part of the portion of tellurium which Muller Von Reichenstein formerly sent to Bergman, in order that the latter might determine whether this body was a particular metal or no. Combinations of tellurium.

I found

Combinations of tellurium.

I found this piece did not contain any foreign metal. I caused it to be dissolved in the nitric acid in a phial correctly weighed: I afterwards caused the acid to be evaporated, and I heated the residue gently, until it would give out no more nitrous vapour. There then remained in the phial 3·745 grs. of oxide of tellurium; that is to say, 100 parts of this metal, combined with 24·63 parts of oxigen. The oxide of tellurium is volatile; if it is too much heated, it begins to sublimate, and if any cold body be introduced into the phial on which it is heated, the latter wlll be immediately covered with a white sublimate. The oxide of tellurium is easily melted, and afterwards, when it grows cold, takes a yellowish colour, and has a crystalline texture. Heated on a bit of carbon before a bellows, it becomes, first of a citron colour, then orange, and lastly, a fine red: it then melts and enters the carbon, where it is consumed, with an effervescence, forming a small greenish flame.

The oxide of tellurium, which has been fused, does not react on paper of litmus. It requires considerable digestion to dissolve it in the nitric acid, and by which it will not be carried to a higher degree of oxidation. With the sulphuric, nitric, and muriatic acids, it forms salts as neutral as those formed with acids, for the most part with other metals. The muriat is decomposed in part, when treated with much water, which precipitates a sub-muriat, but the latter, notwithstanding its evident base, has the property to turn turnsol very red. The nitrate of tellurium, decomposed to a moderate heat, until most of the acid is driven out, affords a white powder, which, being well washed with water, re-acts as an acid, exactly as we see it happens with oxides of antimony, tin, and, like those, the oxide of tellurium loses that property on being exposed to fire.

If oxide of tellurium is mixed with saltpetre, and then exposed to the fire, the nitrate of potash at first melts, without acting on the oxide; but at the temperature at which the latter begins to liquify, it decomposes the nitrate with a violent effervescence, the nitric acid is then driven out, and the oxide of

Combinations of tellurium.

f tellurium dissolves. After it is cooled, the mass very much esembles white enamel. This dissolves perfectly in boiling vater, and this solution, as it grows colder, deposits a white owder, half crystallized. This powder is a combination of he oxide of tellurium with kali; that is to say, it is a tellurate of kali. This tellurate dissolves a little in cold water, and if t is again dissolved in boiling water, it afterwards separates in he form of a powder in grains. It has a sharp taste, and a little like metal, and acts weakly as an alkali on vegetable colours.

The oxide of tellurium dissolves by digestion in ammonia, and this dissolution deposits, as it grows cold, a white powder, analogous to that just mentioned, and this powder is a tellurate of ammoniac.

If, on a dissolution of muriate of lime or barytes, tellurate of kali was poured, this produced a white precipitate, quite insoluble. These precipitates are of tellurate of lime and of barytes.

The tellurate of kali precipitates sulphate of copper, with a green colour, like a very beautiful emerald. This precipitate is the tellurate of oxide of copper (telluras cupricus). When it is heated, it gives out its water of combination, and becomes black. It is easily fusible, and in that state forms a black glass. Heated on carbon before the bellows, it is reduced with a lively detonation, much like saltpetre, and afforded a pale red metallic button (telluretum cupri). The tellurate of kali precipitates the sulphate of oxide of iron, with a reddish colour: solutions of zinc, mercury, lead, silver, and manganese, with a white colour. The quantity of tellurium of which I was in possession being very small, I was not able to produce sufficient from each of these combinations, to examine them more particularly.

To determine the capacity of saturation possessed by the oxide of tellurium, considered as an acid, I have examined the composition of the *tellurate of lead.* On a solution of neutral acetate of lead, I poured tellurate of kali, and I collected the precipitate thus produced. The portions of acetate of lead, which

Combinations of tellurium.

which remained in the liquor not decomposed, had not changed its state of neutrality, except that it reddened the paper of litmus a little more after than before the infusion of the tellurate of kali. The *tellurate of lead*, being well washed, forms a whitish powder. When heated, it loses its water of combination, and becomes yellowish. At a temperature a little higher it melted and formed a mass semi-transparent, and resembling melted muriate of lead. Heated before the bellows on carbon, this tellurate was reduced with a detonation, and formed tellure of lead. I caused two grammes of tellurate of lead to be dissolved in nitric acid, and into that solution poured sulphate of natron, which precipitated sulphate of lead, which, being well washed, and heated hot, weighed 1·477 grs. The liquid, which still retained a little sulphate of lead, dissolved by the muriatic acid, was precipitated by an addition of caustic kali, until the oxide of tellurium was re-dissolved, and there remained a sulphate of lead not dissolved, which, after being made red-hot, weighed 0·033 grs. Therefore, the whole of the sulphate of lead obtained from two grains of tellurate, was only 1·51 grs. in which we found 1·56 grs. of oxide of lead. It follows, therefore, that the tellurate of lead was composed of 42·2 pennyweights of oxide of tellurium, and of 57·8 pennyweights of oxide of tin. The latter contains 4·105 parts of oxigen, while the former contains 6·4 grains, or $\times\, 135 \times 2 = 6{\cdot}27$; that is to say, that the oxide of tellurium contains twice as much oxigen as the base with which it was saturated. I have here called it oxide of tellurium, and notwithstanding it has operated as an acid in the analysed combination, I have done so, because I do not think it right to give another name to the same substance.

### *Telluretum Hydrogenii.*

Mr. Davy found, in his experiments on this body, that the oxide of tellurium, precipitated by means of kali, always contains a considerable quantity of it, and that the metal which is obtained from it by this reduction, contains kalium. I have repeated this experiment, mixing tellurate of kali with carbon, and

Fig. 19.

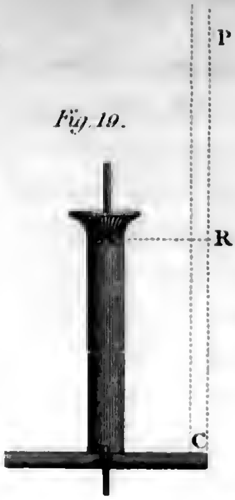



Fig. 20.

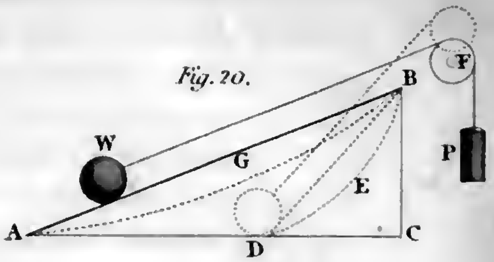



Fig. 1.

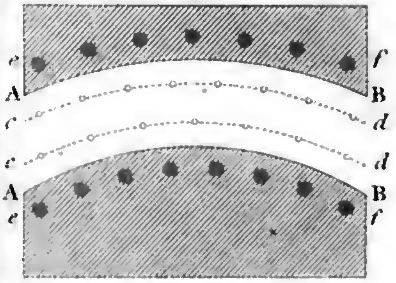



Fig. 2.

Fig. 21.

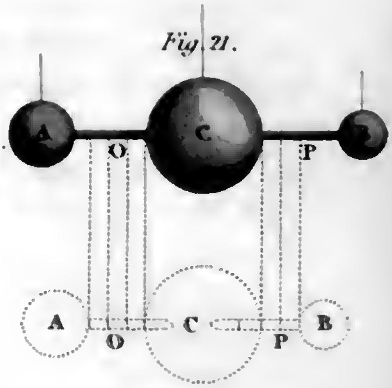



Fig. 22.

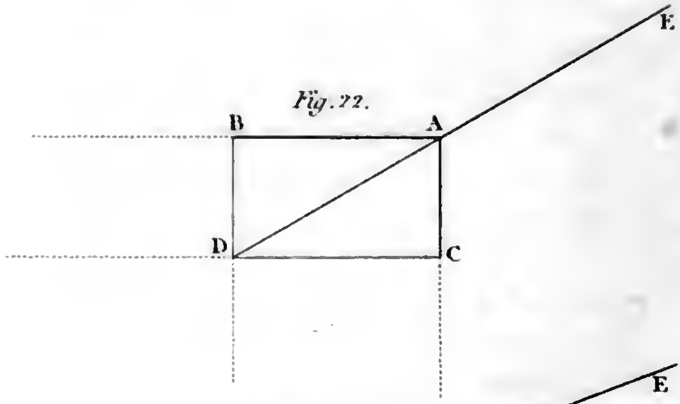
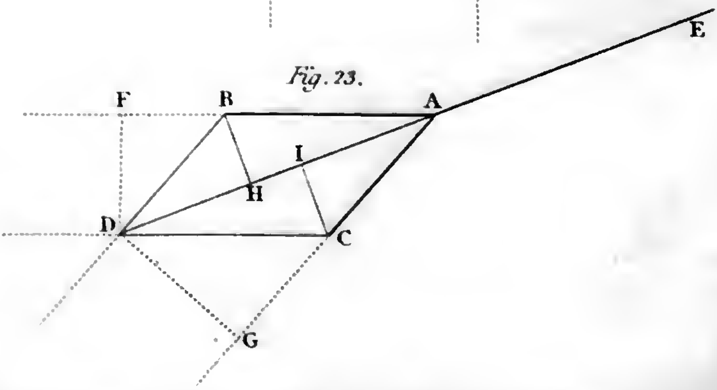



Fig. 23.



and heating this mixture in a small glass retort. The reduction was accompanied with some detonation, and I obtained a black and carbonic mass, which would not liquify. I removed it into a small glass decanter, into which I poured water, which instantly assumed a purple colour, and, in a few minutes, the deep colour of port wine. There remained a portion not dissolved, of black powder, which, being separated from the liquid and heated, took fire, and continued to burn, forming on its surface little globules of metallic tellurium. As the same thing happened with carburet of lead, I concluded, that the powder under the experiments was only a *carburet* of tellurium. As in that experiment there was more tellurium than was necessary for the formation of the hydro-tellurium of kali, it seemed to follow that, in this latter the tellurium was to the kalium in a different proportion than in the tellurate. The dissolution of the hydro-tellurium of kali was lost by some accident, and I procured myself a new quantity in the following manner: I mixed some metallic tellurium with kalium in a small phial, and heated them together; their combination was attended with a lively combustion, as if there had been sulphur or oxigen employed, and the alliance of these two metals produced a metallic ball. This ball I put into a small decanter filled with water, and from which all the atmospheric air had been driven by means of a continued ebullition; after which I closed the decanter. The metal was dissolved by the water, without parting with its gas, and leaving a little overplus tellurium not dissolved. The liquor had a colour of red, tinged with purple, very beautiful. To analyse it, I poured it into a little glass, where I left it exposed to the action of the air. Almost in an instant it was covered with a silvered pellicle of metallic tellurium, which augmented in thickness until it fell to the bottom by its own weight, and was immediately replaced by another. In this manner hydro-tellurium of kali was completely decomposed in less than twelve hours, without the least disengagement of any different substance. From which I think myself authorised to conclude, that, in this analysis there could not have been any loss of tellurium, except the ex-

Combinations of tellurium.

Combinations of tellurium.

exhalation of the telluric and hydrogen gases. This was discovered by Mr. Davy, and I have myself had an opportunity to verify in another experiment (on a penetrating odour of the sulphur and hydrogen gas.) The liquor had deposited tellurium which being correctly weighed, weighed 0·6 grains. The alkaline liquor, neutralised by the muriatic acid, afforded one of oxide of tellurium. I caused it to be evaporated by fire, and I heated the muriat to clear it of the water which adhered. It weighed 1·3 grains; dissolved by water it left a residue of oxide of tellurium, but in too small a quantity to be weighed. 1·3 grains of muriat of kali contains 0·6246 grains of kali, in which there was 0·1102 parts of oxigen. The 0·6 grains of metallic tellurium requires to be oxidated, according to foregoing experiments, 0·147 parts of oxigen. Although this experiment cannot be called absolutely precise, it proves, however, that in the hydro-tellurium of kali, the tellurium and the kalium require the same quantity of oxigen to form of the first oxide of tellurium, and of the last of the kalium. When the mixture of tellurium and kali is dissolved in water, it is necessary that the tellurium should be combined with the whole quantity of hidrogen, disengaged by the water by the oxidation of the kalium; that is to say, that 100 parts of tellurium should be combined with 3·1 grains of hidrogen, and that the tellurium of hidrogen should contain 3 pennyweights of hidrogen.

With another quantity of tellurium of kali I tried to precipitate some solutions of sulphas cupricus, of sulphas manganesus, and of sulphas ferrosus. The first gave a precipitate, black and voluminous; that by the second was brown; and that by the third black. After they were dryed all of them became black. I cannot determine whether they formed hydrotellures or no. On polished steel they did not assume any metallic brilliancy, and by fire they quickly changed to subtellurates.

Tellurium has, therefore, the property of forming three different classes of saline combinations, that is, (1.) salts in which the oxides of tellurium is the base, (2.) salts in which this oxide acts as an acid, and (3.) salts in which the tellurium and hidrogen act as acids. If in this case the tellurium and the oxigen combine

bine with other bases than alkalis, and the alkaline earths; and if the metallic oxides are reduced to *tellurata metallica*, as the sulphur of hidrogen reduces a number of metallic oxides to metallic sulphats; all this remains to be examined.

**Combinations of tellurium.**

The experiments prove, in the end, that there is an affinity in tellurium to the radicals of the saline bases, on which depend as well the acid properties of the oxide of tellurium, as of the tellurium of oxigen. The proportions in which the radical combustibles are found united in all their combinations, are among them in definite ratios, as we know it happens, between sulphur and those radicals with which it combines, as well in a combustible as in an oxidated state. We see, therefore, in this, an ulterior confirmation of what I had said above, as well on the electro-chemical views, as to the *principium aciditatis*.

On the other hand it is evident, that when the tellurium is combined with lead, silver, gold, &c. it acts, in such combinations, the part of an electro-negative body, as sulphur in the sulphurs; these combinations are, therefore, the true tellurata, which, when oxidated, will form tellurates of these metals. Let us, for example, consult the analyses of M. Klaproth, and we shall find that the natural tellures of *Nagay*, analysed by this distinguished chemist, are composed according to these laws. The *weisserz* contains in 44·75 grs. of tellurium, 26·75 parts of gold, 19·5 parts of lead, and 6·5 parts of silver. The quantity of oxigen required to form oxides with these metals, are, from what I shall have an opportunity to say hereafter, for gold 3·21, for lead 1·5, and for silver 0·62, or together 5·23. Now we have seen that 44·75 parts of tellurium forms oxide of tellurium with 11 parts of oxigen, which is the oxigen requisite for oxidating the other three metals for $5·33 \times 2 = 10·66$.

The *Blattererk*, which, perhaps, should be considered tellurate of lead, or 32·2 parts of tellurium, 54 of lead. The first requires to form its oxide 7·965 parts of oxigen, and the latter 4·18 p. which is nearly in the proportion of two to one, so that we may consider the deviation as an imperfection in the analysis. This mineral contains, besides 9 pennyweights of gold, $1\frac{1}{2}$ pennyweight of copper, $\frac{1}{2}$ dwt. of silver, and 3 dwt. of sulphur, in

Combinations of tellurium.

in which the sulphur is proportioned to the total of the other metals; for we know that sulphur, combined with metal, like tellurium, requires twice as much oxigen to form sulphuric acid, as the metal to be basifiable oxide. Now, if we add the quantity of oxigen necessary to oxidate tellurium, and to form sulphuric acid with the sulphur, it requires 10·745 parts, and if we add together the quantities of oxigen necessary to oxidate the other metals, we shall have 5·567, or $5{\cdot}567 \times 11{\cdot}134$, which coincides very well with the calculation. In the mineral named aurum graphicum, we find 60 parts of tellurium combined with 30 parts of gold, and 10 parts of silver. The oxigen necessary to combine with tellurium is 14·8, and that required to oxidate the gold and silver is 4·34. The analysis of Mr. Klaproth having been made on very small quantities, I have reason to think that there is some little error in the result of the analysis, and that these metals are so combined, that tellurium requires four times as much oxigen as the two other metals, that is to say, that these last are combined with twice as much tellurium as in the weisserz.

There yet remains one more question to answer. The sulphuret like telluret of hydrogen, are bodies endowed with acid properties; but the phosphoret and arseniuret of hydrogen, are, as far as we know, entirely exempt, notwithstanding that phosphorus and arsenic are more electro-negative than tellurium. This question is not very easy to resolve, and the only means proper to attempt to gain a solution, appears to me, by making use of the previous discovery of M. Gay Lussac, that gaziform bodies combine either with equal volumes, or that one combines with 2 or 3 times its volume, compared with the others. It appears to me, that this discovery may be employed as a theoretic basis for the laws concerning chemical proportions. From these theoretic considerations, of which I hope to be able to make an exposition in another memoir, it appears that sulphur considered in the form of gas, combines with twice its volume of hydrogen; as it is with oxigen which absorbs twice its volume of hydrogen. Now, if we consider the oxide of tellurium composed of equal volumes of tellurium and oxigen, it follows from what we have just

just determined on the composition of tellurium with hydrogen, that the latter contains one volume of tellurium, and two volumes of hydrogen. But on considering the experiments of Messrs. Thenard and Gay Lussac on the composition of the phosphoret and arseniuret of gaziform hydrogen, as exact, it follows that there is one volume and a half of hydrogen combined with the phosphorus or the arsenic, because in these the hydrogen gas which remains, expands itself to one half the original volume of the compuned gas. Now, if the acid gases necessarily combine with equal volumes of the saline bases, considered in the form of gas, and if the hydrogen, in the acids hydrogeniurets be necessarily in the same proportion to the oxigen of the base, as in water, it follows that sulphuric gas and the tellurate of oxygen contains that portion of hydrogen, but that the phosphoret and arseniuret of hydrogen contain one and half a its volume of hydrogen, and that consequently this moiety of a volume of hydrogen, which does not separate from the combination, must oppose itself to the combination. But it appears, that we are, as yet, too little initiated in these matters, to be able to draw conclusions in which we may venture to place confidence. Combinations of tellurium.

*(The Conclusion has been duly received, and will shortly appear.)*

# METEOROLOGICAL JOURNAL.

| 1813. | Wind. | Max. | Min. | Med. | Max. | Min. | Med. | Evap. | Rain. |
|---|---|---|---|---|---|---|---|---|---|
| 7th Mo. | | | | | | | | | |
| July 20 | E | 29·51 | 29·49 | 29·500 | 75 | 56 | 65·5 | — | |
| 21 | N W | 29.55 | 29 51 | 29·530 | 68 | 57 | 62·5 | — | |
| 22 | S W | 29·55 | 29·50 | 29·525 | 75 | 58 | 66·5 | — | |
| 23 | S W | 29·50 | 29 42 | 29·460 | 75 | 53 | 64·0 | — | 1·00 |
| 24 | W | 29·53 | 29·40 | 29 465 | 74 | 55 | 64·5 | ·48 | ·77 |
| 25 | S W | 29·54 | 29·40 | 29·470 | 73 | 53 | 63·0 | — | ·4 |
| 26 | W | 29·78 | 29·54 | 29·660 | 69 | 53 | 61·0 | — | ·32 |
| 27 | W | 30·07 | 29·78 | 29·925 | 75 | 51 | 63·0 | ·32 | ·10 |
| 28 | W | 30·17 | 30·07 | 30·120 | 77 | 50 | 63·5 | — | |
| 29 | E | 30·07 | 29·95 | 30·010 | 78 | 54 | 66·0 | — | |
| 30 | S | 29·91 | 29·80 | 29·885 | 82 | 60 | 71·0 | ·56 | |
| 31 | N W | 29·95 | 29·91 | 29·230 | 78 | 53 | 65·5 | — | |
| 8th Mo. | | | | | | | | | |
| Aug. 1. | S W | 29·96 | 29·93 | 29·945 | 78 | 59 | 68·5 | — | 5 |
| 2 | W | 29·93 | 29·81 | 29·870 | 79 | 56 | 67·5 | — | |
| 3 | S W | 29·78 | 29·72 | 29·750 | 76 | 51 | 63·5 | ·38 | — |
| 4 | N W | 29·75 | 29·55 | 29·650 | 73 | 49 | 61·0 | — | — |
| 5 | S | 29·53 | 29·42 | 29·475 | 71 | 49 | 60·0 | — | ·26 |
| 6 | N W | 29·85 | 29·53 | 29·690 | 73 | 54 | 63·5 | — | 1 |
| 7 | W | 29·92 | 29·91 | 29·915 | 74 | 49 | 61·5 | — | |
| 8 | S W | 29·89 | 29·87 | 29 880 | 75 | 53 | 64·0 | ·50 | 6 |
| 9 | N W | 30·10 | 29·89 | 29·995 | 70 | 49 | 59·5 | — | — |
| 10 | Var. | 30·10 | 30·07 | 30·085 | 76 | 57 | 66·5 | — | — |
| 11 | S E | 30·07 | 29·98 | 30·025 | 79 | 51 | 65·0 | ·30 | |
| 12 | S W | 29·96 | 29·89 | 29·925 | 80 | 54 | 67·0 | — | |
| 13 | N W | 30·08 | 29·95 | 36·015 | 72 | 48 | 60·0 | — | — |
| 14 | N W | 30·08 | 29·83 | 29·955 | 72 | 57 | 64·5 | ·42 | — |
| 15 | N W | 29·83 | 29·78 | 29·805 | 76 | 51 | 63·5 | — | |
| 16 | N W | 29·82 | 29·80 | 29·810 | 70 | 50 | 60·0 | — | |
| 17 | W | 29·82 | 29·78 | 29·800 | 76 | 52 | 64·0 | — | |
| 18 | N W | 30·05 | 29·82 | 29·935 | 75 | 47 | 61·0 | ·46 | |
| | | 30·17 | 29·40 | 29·799 | 82 | 47 | 63·88 | 3·42 | 2·61 |

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## REMARKS.

*Notes Seventh Mo.* 23. Continued heavy rain for above two hours p. m. with distant thunder. 24. A thunder shower about three p. m. Rainbow. 25. Windy, cloudy, Rainbow, broad diverging shadows on a coloured twilight, with cirrostratus and haze to the S. 26. Fair a. m. At noon began a steady rain, with distant thunder. In the evening, several distinct nimbi, in particular, a well-formed one in the N. E. 27. a. m. Cumulostratus: p. m. rain, with distant thunder: evening distant nimbi, and a rainbow: much colour, with broad shadows, in the twilight. 28. Much dew: cumulus with cirrus. At sunset, a calm air, with large plumose cirri, highly coloured. 29. A clear day, the wind passed from S. to E: twilight brilliant, with dew: the new moon shewed a well-defined disc at 8h. 30m. p. m. 30. Cumulus, with cirrus passing to the inferior modifications. In the evening, on the S. horizon, cumulus, mixed with cirrostratus and haze, the twilight of a pink colour. It lightened frequently before ten at night, very far in the S. E. with the wind S.

*Eighth Mo.* 1. Rain at 5 a. m. succeeded by a close canopy of cumulostratus. At sunset, cirrus with cirrocumulus; twilight opake, somewhat orange coloured. 2. Much the same phenomena as yesterday. 3. Some drizzling showers with wind a. m. sunset very dark, the sky being full of low broken cumuli: night windy. 4. a. m. Windy; with cumulus, which, p. m. inosculated with cirrostratus above it. 5. Rain early, the wind S. In the evening (after several showers) clouds in various modifications, the wind W. with lightning to the S. 6. Much wind at N. W. with cumulus; a shower p. m. 8. Close cumulostratus most of the day. Rain, evening. 9. Wind brisk at N. N. W. a. m. At noon, the upper clouds were perceived not to move with this wind, and at evening it fell calm: there were now in the sky rose-coloured cirri, in stripes, from S. E. to N. W. with cirrostratus and cumulostratus in a lower region: twilight orange, surmounted with rose colour. 11. A stratus after sunset, with cirrostratus remaining above. Small scintillant meteors now appeared, falling almost directly down, and seeming to originate very low in the atmosphere. 13. Cirrus and cirrocumulus abounded. There was a slight shower about noon. 14. Overcast, a little rain after sunset. 16. The *maximum* of the temp. for 24 hours past occurred at 9 a. m. 17. Overcast: windy.

## RESULTS.

Prevailing Winds westerly.

Barometer: greatest observed elevation, 30·17 in.; least 29·40 in.
Mean of the period 29·799 inches.

Thermometer: greatest height 82°; least 47°;
Mean of the period, 63·88°.

Evaporation, 3·42 in. Rain 2·61 in.

L. HOWARD.

TOTTENHAM,
25th, *Eighth Month*, 1813.

## VII.

*A Memoir on the Specific Heat of the Gases. By Messrs.* F. Delaroche *and* Berard. *To which the Prize proposed by the Class of Mathematics and Natural Philosophy of the Institute of France, in the year* 1811, *has been awarded. Abstracted by the Authors.*

*(Continued from p. 284, Vol. XXXV)*

### Section II.

*Apparatus used to cause a regular current of hot gas to pass through the Calorimeter*

Apparatus and Experiments to determine the specific heat of the Gases.

IN order to obtain a regular current of gas, we made use of the gazometer of Wollaston; but in order to heat the calorimeter to the point at which its temperature would have been stationary, very considerable quantities of gas would have been required, which would have in some instances been very expensive, and at all events the dimensions of the gazometers would have required to be very great. To obviate these inconveniences, gazometers were constructed of the same kind, and so disposed as to afford two similar currents of gases; and these instruments were so disposed, that the current from one of them was admitted into the other, without disturbing the regularity of the current. The gas from one gazometer, before it arrived at the second, was made to pass through a tube of more than one metre in length, included in another larger tube in which the vapour of water was continually circulating. In this tube the gas was heated, whence it was passed through the calorimeter, and thence into the second gazometer. When the first gazometer was thus quite emptied, the gas was passed back again, and by means of cocks properly di-persed, again became heated in its course through the tube surrounded by boiling water, and was a second time passed through the calorimeter.

meter. By repetition of these means with the quantity of gas required to fill one of the gazometers, it was possible to keep up a current of hot gas through the calorimeter for several hours*.

The apparatus was so disposed that the calorimeter was in a separate room, the door of which was seldom opened, and in which, consequently, the air not being agitated, preserved a temperature varying very little.

## Section III.

### *Means to discover the Quantity of Heat which is lost by the Current of Gas in passing through the Calorimeter.*

Method of discovering the heat lost in passing through the calorimeter.

In heating the gas by the process described above, it is evident, that whether it is used at the temperature of boiling water, or whether it remains a little below it, it will nevertheless acquire a constant temperature. It seems easy, on a first view, to determine that temperature by placing a thermometer in the centre of the tube through which the gas passes; but on a little attention we shall find, that the thermometer thus placed would indicate a temperature lower than that of the current. In fact, the outside of the tube being always colder than the current of gas which passes through the centre, must consequently act by radiation on the bulb of the thermometer, and lower its temperature. When a thermometer, with the bulb gilt and polished, has been placed in the centre of the tube, and which has been less influenced by the radiation, it has been kept above a degree higher than the common thermometer. This fact evidently proves the influence of the coating, and consequently the difficulty of determining, by means of a thermometer, the temperature of the current passing through the tube.

This difficulty in appreciating the temperature of the current

* The original Memoir, in the Annales de Chimie, indicates the means we adopted to prevent any alteration in the gas during several hours experiment.

of

of gas, at its entrance into the calorimeter, has induced us to allow the least possible length to that part of the intermediate conducting tube between the tube filled with vapour and the calorimeter; by this means, notwithstanding the causes which might operate to lower the temperature of the thermometer, we have, however, succeeded in keeping it up to a temperature nearly equal to that of boiling water, and as we were certain that the real temperature of the current of gas could not be below that of the thermometer placed in the centre of the tube, nor superior to that of boiling water, we could not commit any essential error in estimating it as equal to the medium between these two temperatures.

On the other hand we were certain, that the current of gas, in passing through the spiral tube of the calorimeter, would lose all the excess of its heat, and would go out exactly of the same temperature as the water with which it is filled. We therefore concluded, that the heat left by the current of gas, was equal to the excess of its temperature at its entrance into the calorimeter, over that of the calorimeter itself.

## Section IV.

*Influence of the tube employed to re-heat the gas on the temperature of the calorimeter.*

Influence of the tube.

There was an inconvenience in shortening the intermediate tube, between the tube filled with vapour and the calorimeter; the latter was heated by direct communication, independent of the heat communicated to it by the gas which circulated in its interior. To diminish the quantity of heat given by this means, we employed for this part of the canal a tube of glass, as being formed of a substance no great conductor of caloric.

Whatever precaution we could use, we could not, however, prevent the calorimeter experiencing some influence from the tube employed to heat the gas: but we endeavoured to ascertain correctly what degree of heat was to be assigned to this cause; and we found it was such as would keep the temperature of the thermometer $2^\circ\ 5'$ above that of the surrounding air.

*(To be Continued.)*

## SCIENTIFIC NEWS.

MR. H. Carlisle, F. R. S. F. A. S. Professor of Anatomy in the Royal Academy, and Surgeon to the Westminster Hospital, will begin his Course of Lectures on the Art and Practice of Surgery, and the Sciences connected therewith, on Tuesday, October 12th, at 12 o'clock, at his house in Soho Square.

The introductory discourse is open to all professional students, and the subject to be continued on Tuesdays and Thursdays, at the same hour.

The diseases and accidents allotted to the province of surgery will be amply treated of, and illustrated by cases from the Lecturer's experience. A compendious view of the animal economy will be adduced to illustrate the several processes of disease and of recovery.

The operations of surgery, and the anatomy of the affected parts, are to be demonstrated.

The following arrangements have been made for lectures, at the Surrey Institution, for the ensuing season :

Mr. J. Mason Good, on the Philosophy of Physics, to commence on Friday, the 5th of November, and to be continued on each succeeding Friday. Dr. Thompson, on Chemistry, to commence on Tuesday, the 9th of November, and be continued on each succeeding Tuesday. Mr. Bakewell on Natural and Experimental Philosophy, will commence early in January, 1814 ; and Dr. Crotch on Music, early in February, 1814.

On Monday, October 4th, in George Street, Hanover Square, at the usual morning hours, the Courses of Lectures will

will recommence, viz.—On Materia Medica, and Practice of Physic, at 8 o'clock; and on Chemistry at ¼ after 9, by George Pearson, M. D. F. R. S. Senior Physician of St. George's Hospital, of the College of Physicians, &c.

---

Dr. Roget will commence his autumnal Course of Lectures on the Practice of Physic, at the Theatre of Anatomy, Great Windmill Street, on the first Monday in October.

---

Mr. William Thomas Brande, F. R. S. &c. will give a Course of Lectures on Chemical Philosophy, at the Theatre in Windmill Street, to commence on the second Tuesday in October, and be continued on every Thursday, Saturday, and Tuesday, till the month of May.

---

*Middlesex Hospital.*

The autumnal Course of Lectures on Midwifery, &c. delivered by Dr. Merriman, Physician-Accoucheur to this hospital, and to the Westminster General Dispensary, will commence on Monday, October 11th, at ½ past 10 o'clock.

---

Mr. J. Singer has put to press Elements of Electricity and Electro-Chemistry. This work will comprise a summary of the present state of electrical knowledge, including the subject of Voltaic Electricity or Galvanism, and will contain many original experiments, and a description of some novel articles of apparatus.

---

The Memoir of F. on the Geological System of Warner, will appear in our next.

A

# JOURNAL

OF

## NATURAL PHILOSOPHY, CHEMISTRY,

AND

## THE ARTS.

*NOVEMBER*, 1813.

### ARTICLE I.

*Introductory Letter to the ensuing Article upon the Geological System of Werner.*

*To Mr. Nicholson.*

SIR,

IF the opponents of Werner in this country have complained of the tone which has sometimes been adopted by his disciples, it has, on the other hand, been said, perhaps with equal justice, that the doctrines of a mineralogist of such eminence have been often canvassed in a manner too much approaching to contempt, and not unfrequently censured without being understood. In the enclosed paper, which was written a considerable time ago, it is hoped that the subject is dispassionately treated; and if the writer has failed to comprehend the system, it has not been from inattention. Conduct of the disciples of Werner and their opponents.

Besides the general reasons for the correct diffusion of doctrines that have obtained celebrity, there is a further ground on which a popular view of the Geology of Werner is at present to be desired. The progress of the subject has, of late, been very rapid; and the system must undergo considerable modi- Advantages of a popular view of his system.

fication to adapt it to recent discoveries. It will not, therefore, be uninteresting to have upon record, in a form easily referred to, such an account of it as may shew hereafter the changes it may be thought to require, and enable us to judge how far they are consistent with the principles upon which it was originally founded.

F.

*September*, 1813.

---

*On the Geological System of Werner:*

The facts which indicate the state of the globe are no less important than the theories of formations.

Although the comparative merit of the *Neptunian* and *Volcanic* theories of the earth has been, for several years, a subject of controversy among the mineralogists of Britain, it was not until long after the geological doctrines of the celebrated Werner, grounded on the former theory, had been promulgated on the continent, that they became an object of attention in this country; and even at the present day, the spirit of debate appears to have confined the views of scientific mineralogists, in a great measure, to speculations respecting the *formation* of minerals and rocks, to the neglect of the more important part of their subject, the facts presented in the actual appearances of the globe—a department of knowledge, for the late advancement of which we are highly, if not principally, indebted to the industry of Werner.

Werner has not himself published his researches. Dr. Jameson's Geognosy is the first English publication;

The literary character of Werner himself has been, perhaps, another cause of the imperfect diffusion of his system; from indolence or singularity, he has always been averse to the preparation of his writings for the press; and the results of his geological researches having never been made public, except by lectures, our own knowledge of them has necessarily been derived from borrowed publications of his pupils, of which the majority have been erroneous, and none complete. It was not, indeed, until the appearance of Mr. Jameson's Treatise on Geognosy, that *any* correct view of Werner's method was to be found in our language*; and that work, whatever may be

* The account of the method, published in Dr. Thomson's System of Chemistry, was avowedly derived from Mr. Jameson.

its

its literary character, is probably to be regarded as the best elucidation of the subject that has hitherto appeared.

The object of Mr. Jameson, however, has been rather to promulgate the doctrines of his master, than to discuss their truth: on several questions, where the opinions of Werner are calculated to excite considerable doubt, he has either not given any statement of the difficulties that attend them, or passed very lightly over the arguments by which they are opposed; and though his book has great substantial merit, it must be acknowledged, that the style and arrangement of it are singularly ill calculated to render popular the method which he expounds.

but is not calculated to render that method popular.

It may not, then, be unuseful to state, in language divested of technical peculiarity, the leading facts which constitute the body of Werner's geognostic system, and to discuss briefly some of the theoretic doctrines which he has deduced from them, referring for authority on both divisions of the subject, chiefly to Mr. Jameson's publications, and, in a few places, to the writings of other pupils of Werner, who have distinguished themselves as advocates of his opinions.

Hence it is desirable to give the leading facts in language less technical.

If *Geology** is to be defined "that science which teaches us the structure, relative position, and mode of formation of the mineral masses of which the crust of the earth is composed," a *System of Geology* may be considered as comprehending, 1st. a description of that structure and position, and an arrangement of the rocks which constitute the exterior of the globe; 2dly, a theory or series of deductions by which the appearances so described are to be explained; and, as subordinate to the former, systematic nomenclature and descriptive language, in which the characters and relations of rocks are to

A system of geology should contain, 1. Descriptions. 2 Theory or deductions.

* The term *Geognosy*, invented by Werner to distinguish, as he states, rational science from mere speculation, upon this subject, has been adopted in Germany and France; but the acceptation of the word already established seems, at least in this country, to have been sufficiently correct to render the introduction of a new one quite unnecessary.

Term geognosy needless.

Contrast of talents to describe, and talents to theorize.

be conveyed. It is evident, that an author extensively acquainted with the facts presented by the structure of the earth, and skilled in the art of communicating them, may yet fail eminently in theorizing ; a task for which far different powers are required. The very habit of attention to minute circumstances, which the practice of describing natural bodies must induce, has a tendency to disqualify for the freer movements of speculation ; and the concurrence of precision in detail, with soundness of general views, is but seldom to be expected in the history of the human mind. For Werner, however his admirers claim the praise of excellence in both departments of his subject, and the correctness of his geological theory, is said to be not less remarkable than his acknowledged practical skill in the distinction of minerals, and the developement of the relative positions of rocks.

Werner excels in collecting the facts.

It is to his *geognosy*, indeed, that Werner is indebted for the least disputable part of his reputation ; in "*oryctognosy*" Haüy is a formidable antagonist, but in geology, regarded as a body of fact as well as speculation, there is no rival system to that of the Freyberg school, and the method of investigation which it proposes, has been the subject of the highest praise to several observers well qualified to estimate its value. Humboldt was guided by it in South America ; D'Aubuisson, a native of the country which has produced the system of Haüy, has ably supported the Wernerian doctrines ; and in some recent official publications by the National Institute of France, geology is said to be at present elevated to the rank of a real science, and that elevation is distinctly ascribed to Werner*.

Highly praised by Jameson.

To no person, however, has this geognostic system been the theme of such unqualified applause as to Mr. Jameson, of Edinburgh, who has, throughout his various publications, appeared to consider its decisions as without appeal, and by whom it had been announced in the first volume of his work on mine-

* "Discours sur les progres des sciences, &c. depuis 1789---1809 ;" and "Rapports, &c. sur les ouvrages admis au concours pour les prix decennaire, &c."

ralogy,

ralogy, as "that most splendid specimen of investigation, the most perfect in its kind ever presented to the world*."

But, though the labours of the author of the system have justly given celebrity to his name, their value is certainly not such as to entitle him to this elevated praise. The credit undoubtedly belongs to Werner of having given a new character to the language of mineralogy, though his work on the external characters of minerals, is by no means equal to the Philosophia Botanica of Linnæus, with which it has been injudiciously compared; he has also much enlarged our knowledge of the simple minerals, and has formed a valuable foundation for the scientific history of rocks: but his speculations are seldom successful; in some instances they are almost extravagant—and his *System of Geognosy* can, at the best, be considered but as a temporary magazine of knowledge, hereafter to be employed in the formation of a more philosophic structure.

Distinction: that the labours of Werner entitle him to high celebrity, though his speculations are seldom successful.

The following pages will contain a statement, 1. Of the facts developed by Werner's examination of the earth; and 2. Of his theory of the formation and disposition of rocks; and finally, some brief remarks upon the classification and language which he has adopted or devised.

In the present memoir the facts and theory of Werner will be stated, with remarks.

"At first sight," to use the words of Mr. Jameson, "the solid mass of the earth appears to be a confused assemblage of rocky masses, piled on each other without order or regularity; where none of those admirable displays of skill and contrivance which, in the structure of animals and vegetables, so powerfully excite our attention, and claim our admiration, are to be observed." But a more attentive examination soon discovers, that the uniformity in the workmanship of nature, of which we have an instance in the character and composition of the mineral species collected in the most distant quarters of the globe, extends not only to the combinations of those substances in rocks, but also to the structure and relative position of the masses in which these combinations are assembled; a

Though the structure of the earth seems, at first sight, without order, examination shews, that the characters, composition, and even situations, of mineral masses, are alike in the most distant regions.

* Introd. p. xxii.

fact

fact of the utmost interest and importance, for the distinct knowledge of which, however probable it may have appeared from analogy, we are indebted principally to Werner and his pupils. His own observations in various parts of Germany, those of D'Aubuisson in France, of Humboldt in South America, of Mr. Jameson in Scotland, have proved, that rocks, identical in characters, present themselves in all these regions, and, to all appearance, in the same relative order of succession. The basalts of Germany, Auvergne, the Isle of Bourbon*, of Scotland, and of South America, the granite of India, Scotland, Siberia; the clay-stone and compact felspar of the Pentland hills, near Edinburgh, and those of Saxony, are all, respectively, alike. And greenstone and clinkstone were by Humboldt† found to exhibit, on the lofty ridges of the Andes, the same features of external aspect and position, as on the summits of Saxony and of Scotland.

This interesting fact was established by Werner and his pupils. Instances.

The various relations of rocks thus universal, become, therefore, the object of very interesting inquiry, as they must be attributed to the agency of causes uniform and extensive in their operation. It is to Werner, also, that we are indebted for having pointed out the best method of investigating these relations, and for the developement of certain principles respecting them, which must be admitted to be of very general application, if it be denied that they hold universally throughout the surface of the globe.

Werner also pointed out and used the best methods of investigating these general relations of rocks, &c.

The leading fact to which this method directs the attention, is the *order of superposition* in the masses constituting the exterior of the earth. It has been observed, that certain strata, or masses of rock, are generally found together in groups which have a certain unity of structure and position; or, in the words of Mr. Jameson, in "determinate assemblages of similar and dissimilar rocks, each of which is characterized as an independent whole by external and internal relations." These assemblages Werner has denominated "formations; *simple* when the same species of rock constitutes the

Leading fact; order of superposition.

Masses called formations; simple

* D'Aubuisson sur les basaltes de la Saxe, p. 76.

† Tableau Physique, &c. p. 1, 2, 3, 4.

whole

whole of the formation; *compound* when dissimilar rocks occur in one assemblage. Of the groups which thus exist, some are found with strata parallel to those on which they rest; in others, the superior strata disposed at various angles with respect to those beneath them; whilst in others again the incumbent mass is placed across the ends of the inferior strata, or its position is "unconformable and overlying." In the technical language, then, of Werner, or of Mr. Jameson, "the crust of the globe consists of rock formations of different magnitudes, laid over each other in certain directions;" and his doctrine is, not that in all places all the formations hitherto discovered, have existed, or will be found; but that where they do occur, or in whatever number, their relative *order* always is the same. His investigations have accordingly been directed to determine what these "formations" are, and what the order of their occurrence throughout the globe; and it is the result of these inquiries which his "System," as edited by Mr. Jameson, is intended to convey.

and compound.

The relative order of superposition in compound formations does not vary.

The observations of Lehman first pointed out the great distinction which exists in nature between rocks containing the remains of organized bodies, and those in which no such productions are to be found; and it was soon remarked, that rocks of the latter description are further distinguished by several peculiarities of structure and position. They pervade the whole of the globe, and are uniformly placed beneath all other rocks in strata vertical or very much inclined; their structure is, in most instances, crystalline, and there occurs among their components scarcely any matter that appears to have been mechanically divided. Of the rocks exhibiting these characters, which comprehend several "formations," Werner has constituted a class to which he gives the name of *primitive*, from the supposed priority of the period during which they were produced; a remarkable subdivision being derived from their relative position; for granite, gneiss, mica slate, clay slate, with some others of less importance, occur with a "conformable" arrangement of their strata; whilst primitive trap, the newer serpentine, the second porphyry, sienite and the newest granite,

Distinction of rocks containing organized remains, &c.

Rocks containing none are universal; lowest in situation; strata nearly vertical; crystalline structure; with no mixture of fragments.

They are called primitive formations

nite, are placed above the rest in masses "unconformable and overlying."

Granite three formations.

Of the primitive rocks, *granite* is the most important, and of that compound three formations are said to have been ascertained. The first or oldest of these is placed decidedly below all other rocks, the second occurs only in veins in the first formation "that traverse the oldest granite, but never reach any of the newer rocks;" and the third "appears to be amongst the newest of the primitive rocks, always resting on some of the older, and usually in an unconformable and overlying position. It sometimes, also, (continues Mr. Jameson) occurs in veins that *shoot from the rock,* (a phrase not easily understood) or in veins that are not connected with any rock beyond the strata which they traverse." This third formation affords the mode in which the Wernerians explain the existence of those granatic veins, apparently connected with subjacent masses of granite that occur in certain schistose rocks, and which are still the object of much interesting inquiry and debate.

Transition rocks; less crystalline; contain simple organized bodies; and mechanically divided matter.

Next in order above the primitive rocks occurs a series of formations, in some respects resembling them, of which greywacke, greywacke slate, and limestone are the most remarkable, the last-mentioned substance not having appeared before in any considerable quantity. The separation of this series from the rocks of the primitive class, with the newest of which they had been confounded, is due to Werner, who has founded his distinction on their exhibiting much less of a crystalline structure, their containing organic remains, and the occurrence in their composition of mechanically divided matter. The remains of organized bodies, however, are found in them in very inconsiderable quantity, and are confined to those of beings of the lowest order; from which circumstance Werner infers, that the period of their formation was that during which the earth was passing from its "chaotic to its habitable state;" and he has accordingly denominated them "*transition rocks.*"

Flœtz formations contain compound or-

In the formations which follow those last mentioned, the remains of organized beings occur in progressively increasing quantity,

quantity, and they are found to rise from the lowest order to those of animals and vegetables of higher rank and more complicated structure. The rocks thus marked are generally stratified, the strata are commonly *flat* or horizontal, and Werner has given to the class in which he has disposed them the name of "Flœtz," from a German word, expressive of these circumstances.

ganized bodies and lie in strata flat or approaching to level.

Bituminous substances first make their appearance in this series of rocks; the small proportion of inflammable matter that occurs in the newer primitive masses being carbon uncombined with bitumen.

Bitumen first appears in these.

The most important formations of the "Flœtz" class are, two of sandstone, two of limestone, the same number of gypsum, and that assemblage of beds of sandstone, coal, slate-clay, and other substances on which the ill-chosen name of "the independent* coal formation" has been bestowed; to which must now be added, that of the vicinity of Paris, recently described with great precision by Cuvier and Brongniart†.

Flœtz class, 2 sandstone, 2 limestone, 2 gypsum, and the independent coal formation.

The greater number of the British coal districts afford examples of the "independent coal formation," and the agreement of their structure with tracts of the same formation in various parts of the globe, confirms the fidelity of Werner's observations. The descriptions of the coal districts of Scotland by Williams, and of South Wales by Martin‡ neither of whom, it is probable, had heard even the name of Werner, accord precisely with what has been observed in Germany and France.

The coal districts in various parts of the globe shew the fidelity of Werner's observations.

* *Eigentliche* is the German word which seems rather to imply singular, particular. The term *proper* is also given for it by Mr. Jameson, but he always uses "independent."

† The memoir of these writers, in which this country has been described, is unquestionably one of the most valuable geological publications that has lately appeared, not only as it explains the structure of a very interesting district, but as illustrating the important principle that the more accurate distinctions of zoology can be combined in geological inquiry, with the characters of rocks and their relations.

‡ Phil. Trans. 1806.

The

Remarkable regularity and extent of the Flœtz rocks.

The regularity and extent of the strata of "Flœtz rocks," are often very remarkable. Cuvier and Brongniart were enabled to identify, in the Paris formation, a bed of slaty-marl, not two feet in thickness, but every where presenting the same characters and relative place throughout a space of more than ten French leagues in length and four in breadth; and Mr. Jameson has mentioned a bed of coal, "lately discovered in the second Flœtz limestone, which has been traced for three hundred miles*."

Newest Flœtz trap.

But besides the rocks with parallel and nearly horizontal beds, there is placed in this class another assemblage of considerable importance, the masses of which are "unconformable" in their position, and which occur most frequently in detached and broken summits above the rest. This assemblage has been denominated the "newest Flœtz trap," from its connection with the present class, and from the number of rocks of the *trap-family* which it affords, and among its more remarkable members are basalt, clinkstone-porphyry, amygdaloid, and other rocks, the alledged volcanic origin of which has given rise to so much controversy; with coal in vast abundance.

Alluvial rock &c.

Above all the formations hitherto described, are placed the accumulations denominated "alluvial," formed evidently by the agency of water, and some of them arising or receiving additions from the continued action of the elements even under our own observation. These constitute the *fourth class* of Werner's arrangement.

Volcanic rocks.

The last class in the series is that of *volcanic rocks,* which are placed irregularly above all those that precede them. The connection of volcanoes with the newest Flœtz-trap formation, to which it is asserted they are confined, is a striking and important circumstance: but whether they are the agents to which the origin of that formation is to be ascribed, or have themselves originated within it after its completion, is a question still to be decided.

* Geognosy, p. 165. The place where this coal is found is not mentioned.

Such,

Such, then, is a general outline of the series of formation which Werner has ascertained, and the developement of which constitutes the basis of his geognostic fame: it is fair to hear respecting it the testimony of those who have made this system their peculiar study, and a better commentary of this description can scarcely be found than in the following passages of D'Aubuisson. "Si ces masses et formations, dont la nombre est fort considerable, s'étoient trouvées ensemble et dans un grand etat de regularitè, peutetre les talens d'un observateur ordinaire auroient ils suffi pour ces diverses determinations. Mais comme il est rare qu'il y en a plusieurs dans le meme lieu; qu'elles y sont dans un etat de desordre apparent, ou l'esprit de l'observateur a toujours quelque chose a redresser ou a suppleer; qu'il ne peut les appercevoir que sur une petite etendue, et pour ainsi dire sur des points tres distants les uns des autres; il a fallu pour arriver aux consequences qui forment la geognosie, rassembler, analyser, rapprocher, et comparer un bien grand nombre des faits eloignées, et sans le moindre rapports apparent. Ce sont de pareils rapprochements qui cônstituent les decouvertes dans les sciences; et ce n'est guere qu'aux hommes de genie qu'il est reservè de les faire*." — Commendation of Werner by D'Aubuisson.

To proceed, however, with the *theory* which has resulted from the "rapprochements" here so much extolled. — Theory.

The solid matter of the present surface of the earth is derived, according to Werner, from the contents of a fluid which originally surrounded the globe, and held in chemical solution the various elements of fossils; and the diversified accumulations of rocks are ascribed to depositions from this fluid, modified by successive alternations of retreat and rising. — The solid matters were deposited from a fluid, and modified by its retreat, &c.

From these fundamental positions, another necessarily results, which, in the school of Werner, is considered nearly as an axiom, that the order of *superposition* expresses that of the *formation* of rocks. — Rocks were formed in the order of superposition.

The reasoning by which these doctrines are supported, is not, however, very decisive. Mr. Jameson considers the spheroidal

* Annales de Chimie, tom. 69, p. 228, &c.

figure

Original state of the earth inferred to have been fluid;

figure of the earth as a sufficient demonstration of its original fluidity. "This important conclusion," he continues, "was never disputed, the only question has been whether this fluidity was the effect of fire or water," and that the latter was the agent, he proceeds to infer, because "rocks which have been formed or altered by the action of heat, are most distinctly different from those that constitute the great mass of the globe; consequently this fluidity cannot be attributed to the agency of heat."——"The only other agent that we are acquainted with, that is capable of producing it, is water; and we have the strongest evidence, that this has been the active agent"*.

and this fluidity caused by water.

The "evidence" here alluded to, is derived from various appearances of mineral bodies, and from the resemblance of their structure to that of substances known to have been formed from watery solution. As the highest mountains are composed of rocks possessing such a structure, "we naturally conclude, that the ocean must have formerly stood *very high* over the surface of these mountains:"—and "further, as the most elevated mountains are composed of rocks which extend around the whole globe, and must have been formed during the same period of time, it follows irresistibly, that the ocean must *formerly have covered the whole earth, and at the same time.*"

The solids supposed to have been precipitated; and the fluid to have retreated.

The fluid of which the existence is thus supposed to be established, is next presumed to have precipitated the substances which it had held dissolved; but no cause has been assigned for the commencement of this precipitation. From its first depositions, while yet its contents were held exclusively in chemical solution, arose the greater part of the primitive class of rocks; and after their formation a retreat of the incumbent fluid is supposed to have begun; which was affected, according to Werner, by a "gradual diminution" of the water.

Proofs by Werner.

This last opinion, we are told, originated in very ancient times, and was, more recently, supported by Linnæus and other philosophers.—But "it was reserved for Werner to give this theory stability,"—and his investigations were attended with

* Jameson III. p. 73, 75.

complete

complete success, for he discovered, 1st, that the outgoings of the newer (i. e. superior) strata are generally lower than the outgoings of the older, and this not in particular spots, but around the whole globe; 2dly. That the primitive part of the earth is entirely composed of chemical precipitations; that mechanical depositions do not appear until a later period; and that from this point they continue increasing through all the succeeding classes of rocks to the newest or alluvial, which are almost entirely mechanical deposits*."

The diminution of the waters,

The hypothesis of the "gradual diminution of the waters," is now, in its turn, supposed to be established; and it has been employed to account for several of the appearances of the globe.

notwithstanding the difficulty of accounting for it—

"The next question," says Mr. Jameson, "which naturally presents itself, (and certainly it is not less important than either of those that have been mentioned) is the following:—What has become of the immense volume of water, that once covered and stood so high over the surface of the earth?" To this question, however, Werner has given no reply, contenting himself with inferring the existence of the fact, from what he considers irresistible evidence. But on this part of the subject the argument of Mr. Jameson is not a little singular. "We may," he says, "be fully convinced of its truth, *and are so*, although we may not be able to explain it. To know from *observation* that a great phenomena has taken place, is a very different thing from ascertaining how it happened†!" What is here called observation, however, is evidently nothing but deduction from the appearances observed; and so remote from the actual evidence of the senses, that we are even told in another place, that "the important documents for the illustration of this great phenomenon, were not to be sought for in the formations which have taken place within the limits of *human history*†."

is supposed to be an observed fact!

but is only a deduction.

Mechanically formed fragments,

But to proceed;—the mixture of mechanically divided matter with the chemical depositions, would be one of the effects of a

* Jameson, p. 78, 79. † Jameson's Geognosy, p. 82, 83. 78.

decreasing

decreasing depth of the fluid. "As the water diminished in height, it motion increased, its destroying power reached the surface of the earth, the crystalline shoots were destroyed, and thus the first mechanical productions were formed."

Partial return of the fluid

The retirement, however, which had begun, was not without interruption, for extensive masses of rocks free from organic remains, and for the greater part of a crystalline structure, are found reposing upon the ends of the strata supposed to have been first deposited; and it is inferred, that they were produced by a partial return of the investing fluid, after it had so far subsided as to admit of mechanical action on the consolidated masses beneath; but not to such a depth as completely to expose them; and still before the creation of animals and vegetables.

Retreat by which the transition rocks were formed.

A retreat of more importance, and attended with several remarkable events, is then supposed to have commenced. The summits of the original depositions were unveiled; the dry land began to appear; and the quantity of mechanically suspended matter being much increased, the depositions of this period,—the *transition-rocks*—receive from its abundance a new character: and the creation of organic beings having now taken place, remains of these are also mingled with the substance of these rocks.

The retirement of the fluid continued with increasing agitation, and the earth had now progressively become inhabited by animated beings, in greater numbers, and of various kinds; the quantity of their remains in the depositions of the next succeeding period, the "conformable-flœtz-rocks," is consequently more considerable, and these are found to rise from the lowest rank of animated beings, to those of superior order.

Rapid return of the waters above the highest lands.

But after a retreat, which appears to have reduced the waters nearly to their present level, and to have been of such duration, that the land disclosed had been inhabited for a considerable length of time, a mighty revolution is supposed to have taken place; the waters rising again with tumult and rapidity above the highest summits of the land, overwhelming the various beings that occupied its surface, and mingling their remains in confusion

confusion with fragments of the solid matter of the globe. To this great event the rocks of the "newest flœtz-trap formation owed their birth; and it is imagined that these were originally deposited in continuity around large tracts, if not the whole of the earth; though found at present to occur only in detached and broken portions.

"It is evident," says Mr. Jameson, "from the nature and position of these rocks, that they have been formed by a vast deluge; the water appears to have risen rapidly; again to have become more calm; and, during the period of its settling, to have deposited the different rocks of this formation, and lastly, to have retired to its former level with considerable rapidity.---The broken stratification, which is so characteristic of this formation, was occasioned "partly by the rapid retiring of the water. The heaps of trees, the beds of gravel, sand, and clay, and their more frequent occurrence in low than high situations, their constant occurrence in the lower parts of the formation, are evident proofs of the rapid and tumultuous rising of the waters; the calmness of the water is proved by the fineness of the mechanical, and the increasing fineness of the chemical solution according as we approach to the newer members or upper part of the formation." And greenstone, "a precipitate from a state of solution completely chemical," which is usually the uppermost rock, is the most crystalline." (P. 83. 85.) Newest flœtz-trap formation.

The rapid retreat of the waters, after the deposition of the flœtz-trap rocks, must have been attended with very destructive effects, and considerable changes must thence have been produced, upon what had, previously, been the surface of the globe. From the succeeding deposition of the ruins of former rocks, and of other substances still retained in chemical suspension, arose the class of hills denominated *alluviæ*, whose appearances very plainly tell the history of their formation. The latest revolution which the waters seem to have accomplished, was now complete; and their subsidence to their present level, finally exposed a surface, which has been moulded, by the Alluvial formation.

incessant

incessant action of the atmosphere, of rivers, and of the sea, into the forms, which it now exhibits*.

Particular phenomena.

Such are, in general, the features of Werner's Theory of the earth : but several important phenomena remain still to be explained ; among which the appearances of stratified rocks, the inflection and dislocations of strata, the formation of veins, and the eruptions of volcanoes, are some of the most remarkable.

Strata ; chemical or mechanical : inclined or horizontal, &c.

Werner considers *strata,* as "particular and individual depositions from a state of solution or suspension in water ;" the position being influenced by the mode in which they have acquired their solidity. In chemical depositions they would be inclined at various angles, from accidents of crystallization and inequalities of the surface on which that process had begun ;—but depositions from mechanical suspension would be more nearly horizontal. The strata of the older rocks, accordingly, are much inclined, and the most recent are generally the least so :—in rocks deposited during a continued retreat of the fluid, a certain conformity of position is to be observed, but those precipitated during its revolutions, are variously and irregularly arranged with respect to the strata over which they repose ; and sometimes they are even broken and disjointed.

Dislocations.

But further, some assemblages of strata are so disposed at present, that a change from their original position must unquestionably have taken place ; and various appearances indicate their violent, and sometimes even repeated, dislocation.

* In a general view, *Saussure's* ideas of the mode of the earth's formation come very near to those of Werner, but his doubtful expression of them has more of the spirit of philosophy than the decided language of the Wernerian school.—"Nous voyons donc dans les Alpes, la preuve certaine de la catastrophe, ou de la derniere scene du grande drame de notre globe. *Mais nous ne voyons que des indices fugitifs et problematiques,* des actes precedentes; excepte les preuves de crystallizations tranquilles dans les tems les plus anciens, qui ont precedè la creation des animaux; et de depots et de sediments dans ceux qui ont suivi cette epoque, avec des preuves du mouvements violentes, comme la formation des breches, des poudingues, la brisement des coquilages, et la redressement des couches." Saussure Voyages—§ 2303.

Werner,

Werner, however, is of opinion, that in many instances, the position of strata is ascribed erroneously to change, which in fact was the result of their original formation :—where changes actually have occurred, he ascribes them to ruptures, occasioned "by the unequal accumulation of rocky matter at the time of deposition ; by the loss of support, owing to the diminution of the water, by the disucation of strata caused by the consolidation of crystalline depositions ; sometimes by earthquakes, and the softening of strata during long-continued rains*." Hence also the formation of *veins* is accounted for, which "appear to have been formerly open fissures ; and these fissures seem to have been filled, from above, with the mineral substances they now contain." Dislocations and veins.

The wavings, curvatures, and inflections of strata, and of the lamellæ of rocks, are supposed to have originated from the diversified circumstances of crystallization and deposition. But the causes which gave rise to stratification in general, and which determined the original fluid to yield, in some instances, thick and massive strata, in others very thin yet equally distinct ones, sometimes beds of the same material frequently repeated, but as often contiguous strata perfectly unlike in composition, Werner has not attempted to assign ; and they will probably be among the last secrets of nature, which geological investigation will unfold. Wavings, curvatures, &c. of strata.

The existence of *volcanoes* is ascribed by Werner, to the combustion of the coal that occurs in great abundance among the newest flœtz-trap rocks : the fusion of basalt producing lava ;—or, as Mr. Jameson has expressed it, "we have only to suppose an immense body of coal in a state of combustion ; that its outgoing is covered by a stratum of basalt or wacke ; that hollows are formed by the combustion of the inflammable materials ; Volcanos.

* These are in fact the causes assigned for the formation of the fissures now constituting veins, Geognosy, p. 240 :—but they seem to apply equally to the purposes here stated, and no other mode of accounting for the dislocation of strata has been given by Mr. Jameson.

materials; that the superincumbent basalt or wacke is melted by this heat, and flows into these hollows, and that water rushes in on the surface of this melted mass, and occasions its explosion*.

(*To be continued.*)

---

II.

*On the Measure of Moving Force. By Mr.* Peter Ewart.

(*Continued from p.* 97.)

Cases of difficulty in the doctrines of moving force.

It should be observed, that a weight raised to a given height, and velocity generated in a given mass, are two very different effects of mechanical power; but the measure, composed of the pressure into the space through which it acts, applies equally to both of them. When velocity is generated, the mass into the square of the velocity is always in the ratio of the pressure into the space; but when a weight is raised with an uniform velocity to a given height, it has never, I believe, been contended by any one, that the absolute quantity of mechanical power necessary to produce that effect, or the ascensional force, as it was denominated by Huygens, must be as the square of the velocity with which the weight rises. Such a conclusion would, indeed, be quite in contradiction to the principle of the mechanical force being as the square of the velocity generated.

Mr. Smeaton's meaning will appear still more distinctly, perhaps, if we attend to the particular case he was treating of in the passage objected to by the reviewers. His object was to ascertain the mechanical power of a given quantity of water moving with a given velocity. In order to do this, he constructs an apparatus by which it may be de-

* Jameson III. p. 220.

termined

termined to what perpendicular height a known weight may be raised, with an uniform velocity, by the action of that given quantity of water; and he considers the product of the weight multiplied into the height to which it is raised; or, in other words, the pressure into the space through which it acts, as the proper measure of the effect produced. The current of the water being uniform, he first ascertains, (by means of a pump which supplies it,) the quantity which *passes in one minute,* and then he makes various experiments to ascertain the greatest effect that can be produced by that quantity, by merely multiplying, after every experiment, the weight into the height to which it is raised in a minute. Now, the time of *one minute* is taken merely because it is known that a certain quantity of water passes in that time, the effect of which is to be estimated, being produced in the same time. But the time is by no means a necessary element in the estimation of the effect; for the height to which a weight is raised by any other given quantity of the running water, may easily be determined without reference to the time, and the result will be the same as when the time is considered. Let $p$, for example, represent the power, that is, a given quantity of water moving with a given velocity, and $e$ the effect or the product of the weight into the height to which it is raised by that power, without any reference to the time in which it is raised. Let $p'$ be any other quantity of water moving (for the sake of simplicity) with the same velocity, and $e'$ its effect. Now, if the power be equally well applied in both cases, and if we have adopted a proper measure in estimating the effect, we shall have $\frac{p}{e} = \frac{p'}{e'}$. It is obvious, that this equation will constantly be found by Mr. Smeaton's method, and we must therefore conclude, that he has adopted the proper measure of the force.

Cases of difficulty in the doctrines of moving force.

But Mr. Smeaton's reasoning is farther objected to, as follows: " His second general maxim is, that the expence of water being the same, the effect will be nearly as the height of the effective head, or (as it is expressed in maxim third) as the

Cases of difficulty in the doctrines of moving force.

square of the velocity of the water. This conclusion seems, at first sight, quite in favour of the theory of mechanical force, as laid down by our author, and the other supporters of the *vis viva*; and yet we shall presently find, that it is perfectly conformable to the other theory, and to those reasonings of Desaguliers and Maclaurin, which Mr. Smeaton has censured as leading to conclusions altogether wide of the truth."

" Let $c$ be the velocity of the stream, $v$ that of the wheel, A the area of the part of the float-board immersed in the water, $g$ the velocity which a heavy body acquires in one second when falling freely. Then $c-v$ will be the relative velocity of the stream and the wheel, or the velocity with which the water strikes the wheel; and if we take $h$, a fourth proportional to $g^2$, $(c-v)^2$ and $\frac{1}{2}g$, $h$ will be the height from which a body must fall to acquire the velocity $c-v$, and will be $=\frac{(c-v)^2}{2g}$. Wherefore, by a proposition, well known in Hydraulics, the circumference of the wheel is urged by the weight of a column of water, of which the section is A, and the height $\frac{(c-v)^2}{2g}$, and of which the solidity is therefore $A\times\frac{(c-v)^2}{2g}$. Thus far the investigation is applicable to all undershot wheels, and to all hydraulic engines of a similar construction*."

Now, before we proceed to the remainder of this demonstration†, which is grounded on the supposed certainty of this last conclusion, let us see how far this theory agrees with the results of Mr. Smeaton's experiments.

Let $w$ represent the weight of the column, the solidity of which is expressed by $A\times\frac{(c-v)^2}{2g}$. The value of $w$ in Mr. Smeaton's experiments, is easily found, and he has furnished

* Edinburgh Review, vol. 12, p. 124.

† Namely, that the maximum effect must be produced when $v=\frac{1}{3}c$, and that it is proportional to $c^2$.

data

data by which we can determine nearly the pressure by which the circumference of the wheel is urged. Let $p$ represent that pressure; then, if the experiments agree with this theory, we should always have $p=w$. But we shall look in vain to the results of Mr. Smeaton's experiments for this equation. I subjoin the comparative values of $p$ and $w$, calculated from Mr. Smeaton's first table of eight experiments*:

Cases of difficulty in the doctrines of moving force.

*Experiment* 1. $p = 2{\cdot}3w$

2. $p = 2{\cdot}37w$

3. $p = 2{\cdot}15w$

4. $p = 2{\cdot}22w$

5. $p = 2{\cdot}16w$

6. $p = 2{\cdot}11w$

7. $p = 2{\cdot}01w$

8. $p = 1{\cdot}85w$

And in the 27th experiment, p. 115, we have $p = 2{\cdot}7w$.

If these results be correctly stated, Mr. Smeaton might truly say, that he found these matters to come out in the experiments very different from the opinions and calculations of authors of the first reputation†.

It is true, Mr. Smeaton's maxims agree with some of the results brought out by the common theory. His maxims, however, are by no means the most important conclusions which he has drawn from the results of his experiments; neither can I agree with the reviewers in supposing, that he considered

* If Mr. Smeaton's reduction of his fifth experiment, page 112, be compared with the table page 110, it will appear, that he has omitted to include in the quantities set down in the table, the weight of the scale, pulley, and counterweight. In finding the value of $p$, I have, in each experiment, taken twice the weight of the scale and pulley, added to the counter weight, to be equal to 1·37lb. which will be near enough for the purpose of comparison.

It should be observed, also, that if the table had been made out in the same way, the fourth experiment would have given the maximum effect.

† Phil. Trans. 1776, p. 457.

these

Cases of difficulty in the doctrines of moving force.

these maxims to be inconsistent with the common theory. If it were admitted, according to the theory, that the pressure at the circumference of the wheel is always as $A \times (c-v)^2$ we can hardly suppose Mr. Smeaton to have been so little acquainted with the principles of calculation as not to have been aware, that the maximum effect must consequently be as $A \times c^2$. The principle of the *vis viva* agrees still more remarkably with the common theory in cases of rotatory motion generated about fixed axes, as I have already observed at page 117. But although the rotatory force of a body in motion is, according to the common theory, as the square of its velocity, I do not see why that agreement with the principle of the *vis viva* should be brought as an objection against it. The chief object in discussion is to ascertain upon which principle the most consistent explanation of the facts is to be obtained in cases where the two measures disagree.

It appears to me, that Mr. Smeaton's four maxims on undershot water-wheels may all be comprehended in one, expressed thus: *That in cases where the maximum effect is produced, it is nearly as the quantity of water multiplied by the effective head**. But the theory is founded on the supposition, that, *in all cases* the pressure at the circumference of the wheel is as $(c-v)^2$, and if it were so, the maximum effect would, no doubt, be produced when $v = \frac{1}{3}c$. By the mere inspection, however, of the results which I have stated above, it will be seen, that the pressure at the circumference of the wheel is not as $(c-v)^2$ and therefore the maximum effect cannot be produced when the wheel moves with one-third of the velocity of the water.

I have to regret that I cannot at present refer to M. Bossut's experiments on water-wheels. It is observed, however, by M. du Buat, that, according to these experiments, the maximum effect was produced when the velocity of the wheel was $\frac{4}{9}$ that of the water, which corresponds very nearly with Mr. Smeaton's conclusions.

* It should be observed, that the maximum effect was not always produced at the same relative velocity.

From

From that result, M. du Buat concludes, that the pressure at the circumference of the wheel is as $(c-v)^{\frac{5}{4}}$*. After highly commending the experiments and observations of M. Bossut, M. du Buat continues : " Nous avouons néanmoins, à regret, que, quelque nombreuses et variées qu'elles soient, elles ne sont pas encore suffisantes pour être applicables à tous les cas. Ce ne sera qu'après en avoir fait de nouvelles sur le même plan, et en avoir rapporté les résultats à quelque loi d'approximation simple, telle que celle que nous avons exposée, qu'on pourra espérer de donner des règles pratiques propres à guider les artisans auxquels ces sortes de constructions sont abandonnées†." This observation well merits the attention of every writer on theories of hydraulics. Whether we contemplate the number and diversity of the theories which have been proposed, or the still greater number of facts which appear to be beyond the reach of mathematical explanations, it must, I apprehend, be obvious, that approximation by experiment is all that can, in the present state of the science, be reasonably expected in the comparison or estimation of hydraulic forces ; and we have a convincing proof of the great caution with which such approximations should be sought, in the mistake into which this ingenious, persevering, and skillful experimenter has himself been led, by attempting to generalize too far the results of some of his experiments. I allude to his peculiar theory of *non-pressures*. After very reasonably concluding, that, in cases where water is descending, as it were, upon an inclined plane, the bottom of the channel does not sustain the whole weight of the water, he extends that principle as follows : " Si, par une cause quelconque, une colonne fluide comprise dans un fluide indèfini, ou contenue dans des parois solides, vient à se mouvoir avec une vitesse donnée, la pression qu'elle exerçoit latéralement avant son mouvement contre le fluide ambiant, ou contre la paroi solide, sera diminuée de toute celle qui est dûe à la vitesse avec laquelle elle se meut‡." Now this doctrine is obvi-

Cases of difficulty in the doctrines of moving force.

* Principes d'hydraul. vol. 2, p. 356.

† Ibid. p. 360.

‡ Ibid. p. 175.

ously

Cases of difficulty in the doctrines of moving force.

ously untenable. For, when water is moving upon a horizontal plane, we cannot doubt that the plane must support the whole weight of the water. It is never supposed, that a ball loses a part of its weight by rolling upon a horizontal plane, excepting indeed the amount of its centrifugal force from the centre of the earth; but that exception does not apply to the case in question, for the centrifugal force, whatever it is, must, according to M. du Buat's theory, be added to the non-pressure. In confirmation of his theory of non pressures, M. du Buat observes, "Qu'ayant fait mouvoir, à une certaine profondeur, dans une eau stagnante, un tube vertical ouvert par les deux bouts, dont le supérieur étoit hors de l'eau, le fluide s'est maintenue dans le tube, plus bas que la superficie du réservoir, d'une quantité à-peu-près égale à la hauteur dûe à la vitesse avec laquelle il étoit mu*." But he has omitted to take into consideration the cohesion or the lateral action of the particles of the water upon each other, which has since been so well observed by M. Venturi; from whose experiments, and from those of Dr. Matthew Young†, made under the receiver of an air-pump, we may safely conclude, that, were it not for the pressure of the atmosphere, and the cohesion of the particles, there could be no depression in the tube, as observed by M. du Buat: and, had he been aware of these circumstances, he surely would never have reasoned as he has done on the subject of non-pressures. But to return to the subject of water-wheels.

It has been attempted to be theoretically demonstrated by M. de Borda, and afterwards by Mr. Waring, of America, that the force of the water against the wheel is not proportional to the square of the velocity with which it strikes the wheel, but that it is in the simple ratio of that velocity; and that the maximum effect is therefore produced when the velocity of the wheel is half that of the stream.

M. de Borda, in reference to the labours of others, says, "On ne considéroit qu'une seule palette contre laquelle on

* Principes d'hydraul. Vol. 2. p. 156.

† Irish Phil. Trans. Vol. 7. p. 63.

cherchoit

cherchoit la force du choc du fluide ; mais il falloit observer que dans le mouvement dont il s'agit, l'action du l'eau ne s'exerce pas contre une palette isolée, mais contre plusieures palettes à la fois, et que ces palettes fermant tout le passage du petit canal et ôtant au fluide la vitesse qu'il a de plus qu'elles, la quantité du mouvement perdu par ce fluide, et par consequent le choc qu'éprouvent les palettes, n'est plus proportionnelle au carré de la difference des vitesses du fluide et des palettes, mais seulement à la difference de ces vitesses*."

Cases of difficulty in the doctrines of moving force.

Mr. Waring's demonstration is as follows: "If the relative velocity of a fluid against a single plane be varied, either by the motion of the plane, or of the fluid from a given aperture, or both, then, the number of particles acting on the plane in a given time, and likewise the momentum of each particle, being respectively as the relative velocity, the force, on both these accounts, must be in the duplicate ratio of the relative velocity, agreeably to the common theory, with respect to this *single plane*; but the number of these planes or parts of the wheel, acted on in a given time, will be as the velocity of the wheel, or *inversely as the relative velocity*; therefore the moving force of the wheel must be in the simple direct ratio of the relative velocity," and, consequently, the maximum effect must be produced when the velocity of the wheel is half that of the water†.

But this kind of demonstration cannot, I think, be very satisfactory. It leads, I apprehend, to this conclusion, that we may double the power of any undershot water-wheel, (whatever may be its velocity) by merely doubling the number of its floats or planes acted upon by the water. Mr. Smeaton, however, ever found, that no such advantage was to be gained by that means‡.

It must be acknowledged, that the celebrated experiments of D'Alembert, Condorcet, and Bossut, furnished results in

* Memoires de l'Acad. Paris, 1767, p. 274.

† American Phil. Trans. vol. 3. p. 146.

‡ Phil. Trans. 1759, p. 124.

Cases of difficulty in the doctrines of moving force.

confirmation of the common theory. But these were made under particular circumstances; they did not comprehend a sufficient variety of depths and velocities to afford satisfactory conclusions as to the general question, and various deductions of rather an arbitrary kind, were made from the actual pressure before the result which agreed with the theory was brought out.

On the other hand, we have many experiments which are quite at variance with the theory. We may, in particular, refer to those of Don Juan and M. du Buat. The former exposed to a current of water moving with the velocity of two English feet in a second, a plane of one square foot, immersed one foot under the surface, and found that it supported a weight of $15\frac{1}{2}$lb. which is nearly four times the weight it should have supported, according to the theory*. M. du Buat exposed to a current, having the velocity of three French feet in a second, a plane of one square foot, immersed three inches under the surface, and found that it supported a weight of 19·45 liv. which, by the theory, should have been only 5·75 liv†. M. de Prony attempts to account for the results obtained by Don Juan, by the additional pressure occasioned by the surface of the water over the plane being raised higher than the general level of the current. That circumstance, however, can account for a small part only of the difference. M. du Buat explains his experiments by his theory of non-pressures, which I have already shewn to be fallacious.

M. du Buat has described other experiments which are considered by some to accord better with the theory‡. They were made upon insulated veins of water, spouting from the perpendicular side of a vessel against a surface not greater than the section of the vein; and from their results he draws the following conclusions: " Il résulte des expériences qui précèdent, que le choc d'une colonne, ou d'une veine fluide contre

* De Prony Arch. Hydr. p. 394.

† Principes d'hydraul. vol. 2. p. 218.

‡ Ibid, p. 142, &c.

une

une surface de même étendue et directe, est sensiblement égal au produit de cette surface, par la hauteur dûe à la vitesse. L'intensité du choc dépend néanmoins en partie de la liberté plus au moins grande que les filets ont de se dévier aux approches de cette surface ; mais si la veine rencontre une surface plus grande qu'elle, qui l'oblige à changer en entier la direction de tous ses filets, la vitesse perdue, étant par là augmentée, la resistance devient beaucoup plus grande*."

Cases of difficulty in the doctrines of moving force.

But in these experiments, a part only of the vein strikes the surface opposed to it, and the force of that part appears to be equal to the force assigned by the theory to the whole vein.

Of all theoretical propositions, that which was first demonstrated by Daniel Bernouilli, in his Hydrodynamics, page 290, and afterwards more fully by the same author in the Comment. Petropol. vol. 8, page 120, appears to be the most applicable to Mr. Smeaton's cases, and comes the nearest to his results. It is, that when the force of an insulated vein of water is directed perpendicularly against a plane indefinitely large, its pressure against the plane is equal to the weight of a column of water, of which the base is equal to the area of the section of the vein, and the height equal to twice the height due to the velocity of the vein. But the circumstances of this case are not quite the same as those of Mr. Smeaton, and he found the pressure against the plane to be still greater than the weight of a column of twice the height due to the relative velocity of the water and the wheel.

The most important conclusions drawn by Mr. Smeaton from his experiments are (as I have already noticed) not in his maxims; but they are to be found, I apprehend, in the two following observations, which I shall quote in his own words:

1. "It is somewhat remarkable," he says, "that though the velocity of the wheel in relation to the water, turns out greater than one-third of the velocity of the water, yet the impulse of the water, in the case of a *maximum*, is more than double of what is assigned by the theory†.

* Principes d'hydraul. vol. 2, p. 150.

† Phil. Trans. 1759, p. 130.

2. "We

Cases of difficulty in the doctrines of moving force.

2. "We have seen before, in our observations upon the effects of undershot wheels, that the general ratio of the power to the effect, when greatest, was 3 : 1; *the effect, therefore, of overshot wheels, under the same circumstances of quantity and fall, is at a medium double that of undershot;* and as a consequence thereof, *that non-elastic bodies, when acting by their impulse or collision, communicate only a part of their original power;* the other part being spent in changing their figure in consequence of the stroke*."

It was chiefly in this last consideration, that he found the prevailing theory to be defective; for, according to that theory, as it is applied in explaining the collision of bodies, there can be no force spent in producing change of figure; and it is very remarkable, that no succeeding writer has, as far as I can learn, paid any attention to this circumstance.

However much Mr. Smeaton's valuable observations may have been disregarded by authors, they have not been lost to practical men. Before the publication of the paper which I have been endeavouring to defend, several mills had been constructed under Mr. Smeaton's direction, in which his chief object was to apply the water so that less of its force should be expended in producing a change of figure, and consequently more of its force be communicated to the wheel. Although he had obtained by his experiments results which were "more than double of what is assigned by the theory," yet by comparing the *effective* with the real head, he found that nearly half the power was, in many instances, spent in producing a change of figure in the water, before it reached the wheel; and still finding (as stated above in the second observation) that more than half of what remained of the power was spent in the same way by the manner in which it acted upon the wheel; he determined to apply the water in all cases, so that it should act more by its weight, and less by its impulse; and the advantage gained by that improved construction was found to be fully equal to his expectations. It was afterwards so ge-

* Phil. Trans. 1759, p. 130.

nerally

nerally adopted and improved upon by himself and by other engineers in this country, that although undershot water wheels were, about fifty years ago, the most prevalent, they are now rarely to be met with; and wherever the economy of power is an object, no new ones are made. So that all the points in question, as far as they relate to undershot water-wheels, although highly important at the time when Mr. Smeaton wrote his first paper, are now become matters of mere speculative curiosity, and, in this country at least, they can no longer be of any practical use. The question, however, respecting that part of the power which is expended in producing a change of figure, is highly interesting in other points of view, and we shall have occasion to consider it more fully when we come to examine the 6th, 7th, 8th, 9th, 12th, and 13th cases.

Cases of difficulty in the doctrines of moving force.

Dr. Milner, in allusion to Mr. Smeaton's remarks on the theory, observes that, "It is acknowledged, that the experiments which have been made to determine the effects of wind and water mills, do not agree with the computations of mathematicians; but this is no objection to the principles here maintained. Writers generally propose such examples with a view rather of illustrating the methods of calculation by algebra and fluxions, than of making any useful improvements in practice. They suppose the particles of the water to move in straight lines, and to strike the machine with a certain velocity, and after that to have no more effect. As such suppositions are evidently inconsistent with the known properties of a fluid, we are not at a loss to account for a difference between experiment and theory; and therefore it should seem unreasonable to assert, that certain authors of reputation have neglected the collateral circumstances of time, space, or velocity in the resolution of these problems, unless we are able to point out such omissions*." But if the theory be applicable to speculative objects only, why are its conclusions laid down as rules to be adopted in practice? Mr. Smeaton objected to the practical application of the theory by the distinguished authors

* Phil. Trans. 1778, p. 371

which

Cases of difficulty in the doctrines of moving force.

which he quoted, because they omitted to take into consideration circumstances which render that application inconsistent, as Dr. Milner acknowledges, with the facts. When a stream of water strikes a plane opposed to it, a small number only of the particles of the water touch the plane, and unless we suppose these particles to be pressed forward by the water which is behind them, the actual pressure exerted against the plane cannot be accounted for. But that action of the water is not considered in the prevailing theory; and it is omitted even in the corrected theory which has been proposed by M. de Borda and Mr. Waring; they appear not to have considered, that when the number of planes acted upon are increased, the quantity of water acting upon each plane is decreased in the same proportion; neither are the number of planes acted on in a given time, "inversely as the relative velocity," as stated by Mr. Waring.

The Edinburgh reviewers object to Mr. Smeaton's opinions upon more general grounds, at pages 126---7---8, and continuing to reason as if he had understood the consideration of the time to be necessarily excluded in all estimations of force, they truly and eloquently observe, that, "in most instances, time is a very material element in the estimation of an effect, or an event of any kind; and is, of all our resources, that which it most behoves us to economize*."

Now, I apprehend it is obvious, from the whole of Mr. Smeaton's reasoning on this subject, that he was perfectly aware that, in most cases of moving force, if the pressure, the time, and the *manner* of its acting be given, the effects may be found. He observed, however, (as in the two first cases) that the effects were not always in proportion to the pressure and the time of its acting. But he found, that if the pressure and the space through which it acts (or when variable, the fluent of the pressure into the space) be given, the effects may always be determined without reference to the *manner* or the time in which they may be produced; and finding the total amount

* Edinburgh Review, vol. 12, p. 128.

of

of the effects to be, in all cases, in proportion to the product of the pressure multiplied by the space through which it acts, whatever may be the *time* or the *manner* of its acting, he considers that product to be the principle capable of the most general application, and consequently adopts it as the proper measure of mechanical force.

Cases of difficulty in the doctrines of moving force.

With regard to the proper economy of time, I have always understood, that Mr. Smeaton was fully sensible of its value, and most exemplary in his punctual attention to it, in all its various bearings. We can form no notion of velocity, without taking time as an element of it. As far as it relates, however, to mechanical power, time would come under his consideration chiefly in the following manner. If, for example, the object before him was to apply, to the best advantage, a given stream of water in producing a mechanical effect, he would first ascertain the quantity of water passing in any given time, and the height of its fall. He would next inform himself whether the effect to be produced should be continuous or intermitting in its duration. If continuous, he would construct his machine of such dimensions as to receive and apply the power of the stream uniformly and constantly from hour to hour, and from day to day. But if it were required to produce an intermitting effect, he would construct his machine of larger dimensions, in order to avail himself of the quantity of water which might be reserved during the time that no effect was required to be produced; and he would take care to arrange and proportion the whole, so that no more people than necessary should be employed in attending it. In the latter case, the machine would be said to be more powerful than in the former: but the word power, when used in that sense, has no reference to the measure of the effect when compared with the force by which it is produced. The machine, without the moving force, has no power; and when we speak of the greater or less power of a machine, we only mean to say, that we make use of a larger or smaller instrument to convey the moving force. If we have to let off the water from a reservoir, we know that it will be emptied in less time through a large aper-

ture

Cases of difficulty in the doctrines of moving force.

ture or channel, than through a small one; and just so we know, that by a large and strong machine, a given quantity of moving force may be conveyed in less time than by a small and weak one. But if the whole, or any determinate portion of the moving force be properly applied, the whole or proportionate effect, must nevertheless be the same, whatever may be the portion of time occupied in the operation. And the same principle holds good in the application of the elastic force of steam, or of any other moving force, to produce a mechanical effect.

In objection, however, to this, the reviewers observe as follows:

"When it is said, for example, that a bushel of good coals will give to a steam engine the power required to grind eleven bushels of wheat, this must always imply a rate of burning included within certain limits; for the fuel might be applied so slowly that the steam generated would not be of strength sufficient to work the mill; or it might be made to turn so fast, that very little effect would be produced. In the same way, when Mr. Smeaton says, if 1000 tons of water be let out on an overshot wheel, and descend through twenty feet, it will grind the same quantity of corn, at whatever rate it be expended*, the extreme cases of very great slowness, or very great rapidity, must surely be excepted. But if the extreme cases must be excepted, it is a proof that, even in the intermediate cases, the effect is not constant or invariable in its magnitude, though the differences may be inconsiderable; this, at least, is what one would be disposed to infer from that continuity in the variation of causes and effects, to which there is, perhaps, no exception, either among the works of nature or of art†."

To these objections it may be replied, that however slow or quick the combustion of the coals may be if they be effectually burnt, the full quantity of heat must be given out. If the heat be allowed to escape without being communicated to the

* Phil. Trans. 1776, p. 474.

† Edinburgh Review, vol. 12, p. 129.

water;

water; or if, after being communicated to the water, the pressure of the steam be not wholly applied in producing the intended effect, the loss must be owing to practical imperfections in the construction of the apparatus. Such imperfections must exist, more or less, in every apparatus, and they will, no doubt, be greatest in extreme cases. But although the whole heat, or the whole force, can, in practice never be completely transferred from one given object to another, yet there can be no doubt of the real existence of both the heat and the force in their full quantities; and we can form no idea of the portion of time being limited in which the one must be evolved, or the other transferred. Cases of difficulty in the doctrines of moving force.

A water wheel may be made to move with a velocity so great, that almost the whole pressure of gravity shall be employed in generating motion in the *water*; or it may be made to move so slow as to require a wheel of such magnitude to hold the water, that almost the whole of the force shall be exhausted in generating motion in the wheel, and in overcoming the friction of the machine; but the whole moving force is, nevertheless, in both cases, exerted; and it is immaterial to the principle of its proper measure, whether it be applied in generating motion in the water, or in the machine, in overcoming friction or in producing any other known effect of moving force.

If it appear that I have insisted too much on this part of my subject, it should be recollected, that many of the objections which I have been endeavouring to meet, apply not only to the particular cases under consideration, but generally to the whole question at issue. I must acknowledge, too, that I have felt more than ordinary solicitude that the experience and the conclusions of one who has long been looked up to, in this country, as the father of civil engineers, should be duly appreciated.

But it is not necessary, I apprehend, to resort to complicated cases for the purpose of examining the points in question. If the two first cases which I have stated were once distinctly explained and agreed upon, no difficulty would remain in explaining their various and multiplied applications in machinery.

 Although

Cases of difficulty in the doctrines of moving force.

Although these cases comprehend much of what relates, in this question, to rotatory motion, the three following cases apply more particularly to that branch of the subject.

In rotatory motion it is universally admitted, that four times the force is necessary to generate the same angular velocity, or twice the absolute velocity, in the same body placed at twice the distance from the centre of motion; and it is but reasonable to enquire why we must have one measure for rotatory, and another for rectilinear force? That inconsistency, (stated in case 3d) is overlooked in the usual demonstrations respecting rotatory motion; it is, nevertheless one of considerable importance, and it requires explanation. I have already endeavoured to show (p. 139) that the explanation which refers us to the properties of the lever is by no means sufficient. If, however, the product of the mass into the square of its velocity be taken as the proper measure of the force of a body in motion, the explanation is obvious.

The case of the balance beams (case 4th) has been adduced by many authors in proof of the moving forces being as the masses multiplied into their velocities. There is no doubt, that after they have been put in motion, the weights will balance each other the same as when they were at rest; but the question is, whether or not the motion of *n* can be generated by a moving force no greater than that which generates the motion of *m*? If these two quantities of motion can be generated by equal forces, the same forces should generate equal quantities of motion in *o* and *p*; but equal pressures applied to A and C will not produce, in equal times, equal quantities of motion in the respective weights. Mr. Emerson, by neglecting this circumstance, appears to have been led into the error pointed out by Mr. Atwood, which I have quoted at page 128. But if the weights were attached to, instead of being suspended from the ends of the beams, the case would then be one of pure rotatory motion, and would have been included in the 56th prop. of Emerson's Principles of mechanics, where it is demonstrated, that unequal quantities of motion are produced by equal forces

in

in equal times, and where the individual forces are made out to be as the revolving masses into the squares of their velocities. If he had applied the same principles to the solution of the problem quoted above from his treatise on fluxions, he would, no doubt, have brought out the true, instead of an erroneous, result.

Cases of difficulty in the doctrines of moving force.

In his 56th prop. the forces are understood, in the usual way, to be modified by the properties of the lever, and then their relations to each other, and to the squares of the velocities generated, are made out. But it is the pressure only that is modified according to its distance from the centre of motion. The product of the pressure into the space through which it acts, remains the same, whether it be taken at the point where the force acts on the lever, or where the lever acts on the body which is moved. The force of a body in motion cannot be considered greater or less, according to the manner in which it has been produced, and when we see a body in motion, if its mass and velocity be given, we never ask by what kind of lever it has been produced in order that we may judge of its force.

The case of a balance beam was noticed by Sir Isaac Newton, near the end of his scholium to the laws of motion; but it is not clear that he considered that case in the same light in which it has since been taken by Desaguliers and other authors, to prove that the moving forces of the weights are not as the squares of their velocities. It may, I apprehend, with greater consistency, be inferred, that he noticed that case, merely to show, that the pressures of the weights balance each other when they are in motion the same as when they are at rest. It will be seen, when we come to examine the 14th case, that Sir Isaac Newton did not consider quantities of motion to be in all cases in the ratio of the forces by which they are produced.

The 5th case belongs to that class of the effects of force, which are considered by Mr. Atwood to be disproportionate to the forces by which they are produced, which ever way they may be estimated, whether by the mass into its velocity, or by the mass into the square of its velocity. However strange this opinion may appear, it is perfectly correct as far as it is

Cases of difficulty in the doctrines of moving force.

applied to the measure of force composed of the pressure and the time of its acting; for, according to that measure the quantity of force communicated will be always the same, whether it be applied at G, D, or at any other point in A B. The progressive velocity generated in G, will, no doubt, be the same, at whichever of these points the force is communicated; that is, the product of the mass into its velocity *in the same direction* will, in this case, as in all others, be as the product of the pressure into the time of its acting; and according to that measure, the whole effect of the force communicated is found in the progressive motion of the mass, the rotatory motion appearing to be produced without force. The explanation most commonly given of this inconsistency is, that the rotatory motion, consisting of equal quantities of motion in opposite directions, balances itself; but can it be shown, that equal quantities of motion in opposite directions may be produced without force? Such is not the doctrine of Sir Isaac Newton; he certainly understood rotatory motion, as well as rectilinear motion, to be a measurable effect of force. M. de Prony attempts to explain this difficulty, in the application of the prevailing measure of moving force, as follows: "Puisque nous savons que lorsque la résultante des quantités de mouvement imprimées passe par le centre de gravité d'un corps, ce corps, abandonné à l'action des moteurs, n'a aucun mouvement de rotation, il faut en conclure, que le mouvement de rotation n'a lieu que lorsque la résultante des quantités de mouvement imprimées passe hors du centre de gravité. Ensuite, comme le mouvement de ce centre est le même, soit que la résultante y passe ou n'y passe pas, c'est donc autour du point où il est placé pue se fait la rotation, quand il y en a, puisque ce point est le seul qui ne participe pas à cette rotation. Il suit de là que le mouvement de translation est absoluement indépendant du mouvement de rotation, puisqu'il est indépendant de la cause qui le produit, savoir, la direction de la résultante par un autre point que le centre de gravité*."

* Arch. Hydr. p. 176.

But

But how can these two motions be independent of each other, when they are both produced by the same force? The pressure can neither be increased nor diminished without increasing or diminishing, at the same time, the rotatory as well as the progressive motion; and if we attend to the space through which the pressure acts, we shall have no difficulty in finding what part of the whole moving force is expended in producing the progressive, and what in producing the rotatory, motion

Cases of difficulty in the doctrines of moving force.

Let E be the centre of gyration of A and B around G. Draw GF, DH, and EI perpendiculars to AB. On EI take two points K and I, so that EK : KI : : GE : GD. Through K draw KF parallel to AB, and through F and I draw MN. Then if we take GF to represent the progressive velocity produced in G by any force acting at D, KI will represent the rotatory velocity produced in E in the same time; DH will be the whole space through which the pressure has acted; DL will represent that portion of the moving or mechanical force which has produced the progressive velocity; and LH that portion which has produced the rotatory velocity, and we shall have $GF^2 : KI^2 :: DL : LH$. These results are so well known, that it would be superfluous in me to give a demonstration of them here. The same relations of the moving force to the effects, and of the effects to each other, take place whether the force be communicated by impulse or by gradual pressure. For, however sudden the impulse may be, a determinate space must be described by the pressure during its action, and if the pressure be uniform, that space, however small it may be, must consist of two parts, as described in the figure, having the ratio to each other of $GF^2 : KI^2$. If the pressure be not uniform, the fluent of the pressure into the space will bear the same relation which DH bears to the sum of the products of the masses into the squares of their velocities.

*(To be continued.)*

METE-

# METEOROLOGICAL JOURNAL.

| 1813. | Wind. | Max. | Min. | Med. | Max. | Min. | Med. | Evap. | Rain. |
|---|---|---|---|---|---|---|---|---|---|
| 8th Mo. | | | | | | | | | |
| AUG. 19 | N | 30·14 | 30·05 | 30·095 | 71 | 47 | 59·0 | — | |
| 20 | N | 30·14 | 30·11 | 30·125 | 70 | 40 | 55·0 | — | |
| 21 | N W | 30·11 | 29·83 | 29·970 | 68 | 50 | 59·0 | ·26 | — |
| 22 | N W | 29·91 | 29·71 | 29·810 | 65 | 44 | 54·5 | — | ·17 |
| 23 | | 30·23 | 29·91 | 30·070 | 67 | 46 | 56·5 | — | |
| 24 | N E | 30·25 | 30·23 | 30·240 | 69 | 42 | 55·5 | ·24 | |
| 25 | N E | 30·25 | 30·23 | 30·240 | 70 | 52 | 61·0 | — | |
| 26 | N E | 30·23 | 30·15 | 30·190 | 69 | 48 | 58·5 | — | |
| 27 | N W | 30·15 | 30·10 | 30·125 | 68 | 51 | 59·0 | ·25 | — |
| 28 | N | 30·10 | 30·06 | 30·080 | 66 | 53 | 59·5 | — | ·15 |
| 29 | N E | 30·22 | 30·10 | 30·160 | 69 | 52 | 60·5 | — | 4 |
| 30 | N E | 30·26 | 30·26 | 30·260 | 67 | 53 | 60·0 | — | |
| 31 | E | 30·26 | 30·05 | 30·155 | 70 | 53 | 61·5 | ·32 | |
| 9th Mo. | | | | | | | | | |
| SEPT. 1 | S E | 30·05 | 29·85 | 29·950 | 65 | 56 | 60·5 | — | — |
| 2 | S W | 29·90 | 29·85 | 29·875 | 67 | 48 | 57·5 | — | — |
| 3 | S | 29·95 | 29·85 | 29·900 | 75 | 58 | 66·5 | — | |
| 4 | S | 29·85 | 29·75 | 29·800 | 73 | 60 | 66·5 | — | — |
| 5 | S W | 29·75 | 29·25 | 29·500 | 70 | 56 | 6[illegible]·0 | — | ·69 |
| 6 | S W | 29·49 | 29·27 | 29·380 | 68 | 52 | 60·0 | ·61 | — |
| 7 | W | 29·67 | 29·49 | 29·580 | 64 | 44 | 54·0 | — | — |
| 8 | N W | 29·85 | 29·67 | 29·760 | 56 | 43 | 49·5 | — | — |
| 9 | N W | 30·19 | 29·85 | 30·020 | 50 | 41 | 45·5 | — | |
| 10 | N W | 30·24 | 30·19 | 30·215 | 61 | 49 | 55·0 | — | |
| 11 | S W | 30·24 | 30·10 | 30·170 | 70 | 58 | 64·0 | ·55 | |
| 12 | S W | 30·06 | 30·00 | 30·030 | 72 | 51 | 61·5 | — | |
| 13 | N W | 30·06 | 30·05 | 30·055 | 63 | 51 | 57·0 | — | ·18 |
| 14 | N W | 30·18 | 30·06 | 30·120 | 64 | 42 | 53·0 | — | |
| 15 | S W | 30·18 | 30·17 | 30·175 | 70 | 51 | 60·5 | — | |
| 16 | N W | 30·29 | 30·17 | 30·230 | 72 | 50 | 61·0 | ·40 | |
| | | 30·29 | 29·25 | 30·009 | 75 | 40 | 58·44 | 2·63 | 1·23 |

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## REMARKS.

*Notes. Eighth Mo.* 19. Cirrus and circstratus clouds: rather windy. 20. Cumulostratus: windy. Cirrus clouds, very red, at sunset. 21. Windy. 22. Cloudy morning, small rain: showers and wind: rainbow about 6 p. m. the sky richly coloured, and the clouds evaporating. 24. Cumulus during the day: cirrus at sunset; twilight brilliant and coloured with traces of cirrocumulus and of stratus. 25. Overcast with cumulostratus: twilight opake and coloured. 26. Windy a. m. Cumulostratus clouds, the remains of which, at sunset, glowed with a succession of crimson and purple tints on a full orange ground. 27. Windy a. m. a little rain. 28. Overcast: much wind, and at night rain. 29. Cumulostratus clouds chiefly: a shower or two: the twilight luminous, but opake and surmounted by a blush of red considerably elevated. 30. a. m. Cloudy.

*Ninth Mo.* 1. (at Stratford.) Slight shower at evening. 2. Very cloudy morning. 5. Heavy rain after 6 p. m. lunar halo. 6. Rainy morning: high wind. 7. Much wind still: showery. 9. Very fine moonlight night. 12. Abundance of cirrocumulus, gradually lowering, and arranged in close lines from S. E. to N. W. 14. A shower in the evening. 16. About 5 p. m. a *solar halo*, of short continuance: the sky at sunset was (as usual of late) much coloured: there was a considerable diffused redness above the twilight, and some portions of the clouds, even against this, varied from the usual indigo colour to a pale olive green: an indistinct appearance of nimbus in the E. horizon.

## RESULTS.

Prevailing Winds Northerly, with an interruption of some days continuance from the southward, producing for the time a considerable depression of the Barometer, together with elevation of the mean temperature, and rain.

Barometer: greatest height 30·29 in.; least 29·25 in.

Mean of the period 30·009 inches.

Thermometer: greatest height 75°; least 40°;

Mean of the period, 58·44°.

Evaporation, 2·63 in. Rain 1·23 in.

The observations from the 30th of the eighth month to the close of the period are chiefly those of my friend John Gibson, at the Laboratory, Stratford.

L. HOWARD.

TOTTENHAM.
*Ninth Month*, 18*th*, 1813.

III.

*A Memoir on the Specific Heat of the Gases. By Messrs.* F. Delaroche *and* Berard. *To which the Prize proposed by the class of Mathematics and Natural Philosophy of the Institute of France, in the year* 1811, *has been awarded. Abstracted by the Authors.*

*(Concluded from p. 142.)*

Section II.

*Determination of the specific Heat of Gas, that of the Air being taken for unity.*

Apparatus and Experiments to determine the specific heat of the Gases.

THE process we followed in the experiments we have been induced to make on the various gases, being the same for all, and having been sufficiently explained in the preceding section, we shall content ourselves in presenting the results together in a table. With a view to render them more easily compared, we have made some corrections, of which it is necessary to speak:

At first we conjectured, that in the limits of the temperature in which the experiments had been made, the specific heat of the gases did not vary; but that small difference in the temperature had great influence on the measure of the gas, and in order to be able to compare the results better, we have supposed, in the fourth column, that all had been measured at 0°.

To bring the results obtained with the different gases, to the same pressure, it was necessary to know, at least as nearly as possible, the influence of pressure on the specific heat of the gases. For which reason we shall explain, in one of the following sections, the manner in which we have obtained, by calculation, the numbers which compose the eleventh column.

The last column of the table, pointing out the excess of the stationary temperature at which each current would have kept the calorimeter above that of the ambient air, all circumstances

ances being the same, the specific correspondent heats were thus concluded.

| | | | |
|---|---|---|---|
| The specific heat of the air being - - - - - | 1·0000 | - - - - - - - - - | 1.0000 |
| That of the same volume of hydrogen | 0·9033 | or the same weights | 12·3401 |
| Carbonic acid - - - | 1·2583 | - - - - - - - - - - | 0·8280 |
| Oxigen - - - - - - | 0·9765 | - - - - - - - - - - | 0·8848 |
| Azote - - - - - - - | 1·0000 | - - - - - - - - - - | 1·0318 |
| Oxide of azote - - | 1·3503 | - - - - - - - - - - | 0·8878 |
| Olefiant gas - - - - | 1·5530 | - - - - - - - - - - | 1·5763 |
| Oxide of carbon - - | 1·0340 | - - - - - - - - - - | 1·0805 |

## Section III.

### *Determination of the specific Heat of the Gases by another Process.*

If, instead of determining at what point every current of hot gas could maintain the temperature of the calorimeter stationary we had proposed to inquire during what time it was necessary to make each current circulate; or, in other terms, how much of each gas was necessary to communicate to our calorimeter a given number of degrees, supposing that each current lost the same number of degrees in passing the calorimeter; is it not evident that the specific heat of each of the gases would be found in the inverse ratio of the quantity of gas necessary to communicate to the calorimeter the same number of degrees ? This consideration would furnish us with another process to obtain the comparative specific heat of each gas. Besides, an idea was long since entertained of determining the heat which is disengaged under any circumstances, by an analogous process; but there was one cause of error to struggle against, which materially affected the correctness of the results. In proportion as the materials of the calorimeter itself were heated, the air and the surrounding bodies would carry off a part of that heat. All that excess, therefore, which supplied it with caloric, was not employed in raising its temperature; it became, therefore,

therefore, necessary to ascertain the quantity of heat thus lost, and that was almost always very difficult.

It was to obviate this inconvenience, that Count Rumford adopted the happy idea of taking his point of departure not at the temperature of the surrounding air, but somewhat lower, to which he took care to reduce his calorimeter, and to continue his experiment no longer than was necessary to acquire re-heating, by a term as much above the surrounding temperature as that was above the initial temperature. By this means the heating the calorimeter was rendered independent of the loss of heat, which might arise from the air; for if, on the one hand, during the first part of the experiment, this air, being hotter than the calorimeter, affords it any quantity of heat whatever, in the latter part of the experiment, by an inverse reason it takes off a quantity nearly equal; we say nearly equal, for to make it exactly so, it would be necessary that the division of the times in which the re-heating takes place should be equal, which cannot always be.

This ingenious modification has supplied us with the means of determining, with correctness, what quantity of each gas was necessary to communicate to the calorimeter an elevation to a given temperature, supposing that every gas grows cold in the same degree. The results of these experiments will be seen in the table.

N. B.—1°. We ascertained, by two preliminary experiments, made with great care, and which gave results very nearly the same, that in the experiments made agreeably to this process, the tube employed to heat the gas communicated, in ten minutes, to the calorimeter, a heat sufficient to raise its temperature to 0°,190; and as the heat thus afforded to the calorimeter was sensibly proportional to the time, nothing is more easy than to correct the results in that respect.

2. The correction respecting the pressure has been made in a manner analogous to that which has been calculated in the preceding table.

It results, from what we have advanced, that the numbers contained in the last column of this table, are in an inverse ratio

atio of the specific heats, and that consequently the specific

| | | |
|---|---|---|
| eat of the air being | 1·000 by the first process | 1·0000 |
| That of the same volume of hydrogen | 0·893 - - - - - - - | 0·9033 |
| Carbonic acid - - | 1·311 - - - - - - - | 1·2583 |
| Oxigen - - - - - | 0·974 - - - - - - - | 0·9765 |
| Azote - - - - - | 1·000 - - - - - - - | 1·0000 |
| Oxide azote - - - | 1·315 - - - - - - - | 1·3503 |
| Olefiant gas - - - | 1·680 - - - - - - - | 1·3530 |
| Oxide of carbon - - | 0·983 - - - - - - - | 1·0340 |

## Section IV.

*Specific Heat of the Vapour of Water compared with that of Air.*

Aqueous vapour, being an elastic fluid, which exercises a very marked influence on a great number of the phenomena, it is of importance to have some notions of its specific heat; but we find that it is almost impossible to operate on this fluid in a state of purity; for it is essential for experiments of this kind, that the whole of the apparatus should be carried beyond 100°. We must, therefore, when we would seek the specific heat of this vapour, have recourse to its mixture with the air, and it is also necessary that the greater part of the apparatus be in a very hot atmosphere, if we wish the vapour to form a considerable quantity of the mixture. Our process will very easily apply to this determination. It was, in fact, sufficient to examine comparatively at what term the temperature of the calorimeter was kept stationary—at first, by a current of dry air, and afterwards by the same current augmented by all the vapour which it could dissolve at a determined temperature, a condition which was easily accomplished. Specific heat of vapour compared to that of air.

This is the result of an experiment made with that view. A hot current of dry atmospheric air had raised the temperature of the calorimeter to 8°43 above the ambient air, and kept it stationary at that point. The same current, saturated with the vapour of water at the temperature of 39°, all other circumstances being absolutely the same, has constantly kept the

the temperature of the calorimeter elevated 9°53 above that of the ambient air. The difference between these two results was, therefore, 9°53—8°43=1°1 = effect produced by the vapours.

The barometer, during the course of this experiment, was at 0·7594 metres. With this result, if we consult the table of Dalton, we shall find, that the volume of air compared with the vapour was as 15·0 to 1. We have seen that the effect of this vapour was expressed by 1°1; and as, in general, this effect is proportional to the quantity of elastic fluid which traverses the calorimeter, it follows, that a current of vapour equal to the current of air would, under the same circumstances, raise the calorimeter to 16°5 above the ambient air. We may, therefore, from these experiments, conclude, that the specific heat of air being 1·00 - - - - 1·000
That of the vapour of water
is, with the same volume, 1·96 or under the same
weights - - 3·136*.

We are, however, obliged to confess, that the quantity of vapour and its effect being very little, and having been multiplied by 15 to establish the comparison with the air, a small error in the experiment may cause a considerable one in the result.

## Section V.

### *Specific Heat of the Air under different Pressures.*

Specific heat of the air under different pressures.

The form of our gazometers was such, that we could cause air, subject to different pressures, to circulate from one to the other. It was easy, therefore, for us to heat also beforehand currents of air under different pressures, and to determine at what term each of them would maintain the temperature of the calorimeter stationary.

* This specific heat has been calculated on the supposition, that the weight of vapour is to that of air as 10 : 16.—Gay-Lussac, *Annales de Chimie, tom.* 80, p. 218.

The

The following is the result of two comparative experiments ade on the air at two different pressures.

A current of atmospheric air of 35·961 litres in ten minutes, nder a pressure of 1·0058 metres of mercury, in cooling from 2°415, afforded a quantity of heat sufficient to support the mperature of the calorimeter to a term more elevated by 3°703, than that in which it had been maintained without this rrent.

But the same current of air under the pressure of 0·7405, her circumstances being the same, supported the temperature f the calorimeter only 15°423 above that of the ambient air: e proportion of these two numbers may therefore be taken for eir specific heats.

These two comparative experiments, repeated with great care, ave us, after having made all the calculations with respect to e specific heat of the air, subjected to a pressure of 0m.7405, d that of the air subjected to a pressure of 1m.0058, that of : 1·2665.

Taking the medium between the two results, we find that, he specific heat of the air, (at the pressure of 0·7405) eing - - - - - 1·0000 - - 1·0000 hat of the same volume of air, t the pressure of 1·0058) is - 1·2396 and the same weight - 0·9126

If we suppose, that the differences between the pressures are roportional to the differences between the specific correspon-ent heats, we may, by means of the preceding results, re-uce by calculation, the specific heat of the air taken under ne pressure, to what it would be under a different pressure. ut as it is clear, that the suggestion we here announce ap-roaches nearly to the truth, and is applicable to all the gases, follows, that the differences between the pressures must be ery small; this induces us to believe, that we have not made ny sensible error in applying, in the preceding tables, a cor-ection founded on these principles.

SEC-

## Section VI.

### *Determination of the specific Heat of Gas compared to that of Water.*

§ 1.

*First Method.*

Specific heat of gas compared to water.

In the conclusion we have drawn of the proportions of the specific heat of the different gases, we set off with this principle—that this specific heat was proportional to the *maximum* of the elevation of the temperature to which a hot current of each gas would carry the calorimeter; to connect these specific heats with that of water, it was sufficient, therefore, to compare the effects produced by the different gases with that which a current of hot water, so gentle, that its effect should not be much more considerable, would produce.

We had obtained a very gentle current of water by means of a capillary syphon, plunged into a vase full of water at a constant level. This current was heated by passing through a tube filled with the vapour of water, and afterwards passed through the calorimeter. Between the tube which heated this current and the calorimeter, the current passed through a passage of about fifteen centimetres, where a convenient apparatus had been disposed to take the temperature very exactly. On quitting the calorimeter, the water passed by a tube formed to a point, into a graduated tube, which served to measure the rate of the current.

From the experiment made by this process it appeared, that a current of water of 37·750 grammes in ten minutes, having been cooled by 29°072, maintained the temperature of the calorimeter 20°713 above that of the ambient air. In comparing this result with that obtained with the current of air presented by the first table, taking care first to turn the litres into grammes, we find,

The

| | |
|---|---|
| he specific heat of water being - - - | 1·0000 |
| hat of air is - - - - - - | 0·2460 |
| second experiment gave - . - - - | 0·2536 |
| Medium | 0·2498 |

§ 2.

*Second Method.*

The second method of ascertaining the proportion of e specific heat of air to that of water, consisted in deter- ining, by calculation, the real quantity of heat lost in a given ne by the calorimeter, when the current of hot air had ren- red the temperature stationary. It is, in effect, evident, that hen arrived at this point, the calorimeter loses a quantity of at equal to that communicated by the current. These are e bases of the calculation, applied to the experiment made on e air, and presented in the first table.

Second method.

We know exactly the quantity of copper and tin employed the composition of our calorimeter, and the quantity of water hich it contained. Its whole mass contained as much heat as 66·8 grammes of distilled water.

On the other hand, we have ascertained by a very careful periment, that if, after the current of hot atmospheric air has used the temperature of the calorimeter to rise to a stationary int, we stop the current and leave the calorimeter to the e air, it will lose, in twenty minutes, a heat capable of low- ng its temperature 2°887. This number of degrees is not quantity sought, because, in that experiment, the quickness the cooling will decrease every instant. But this experi- ent, by means of an easy calculation, may lead us to discover quantity of heat lost, if the quickness of the cooling had, ing the twenty minutes, been the same as it was in the t instant. This quantity being represented by S, we have

$$S = A \text{ log. hyp. } \frac{A}{B}.$$

In

In this equation, A marks the excess of the temperature of the calorimeter over that of the surrounding air in the first instant of the experiment, and B that excess at the end of twenty minutes, supposing the calorimeter left to itself. In making the application to the experiments made on the air, we see, by by the first table, that A=15°734, and from the preceding experiment A—B = 2°887, and consequently B = 12°847, which gives S = 3°1895. Another experiment, calculated in the same manner, gives for S = 3°2089. The medium of these two numbers is S = 3°1992, which marks the degrees of coolness of the calorimeter in twenty minutes, if the rate of cooling had been during all that time the same as in the first moment, we should have $\frac{S}{2}$ = 1°5996 for the same decrease of heat in ten minutes.

The current of hot air therefore communicated to the calorimeter in ten minutes a heat capable of raising its temperature to 1·5996. Now, the quantity of air which traversed the calorimeter in ten minutes was, by this table, 35·99 litres, or 46·860 grammes, and the loss of heat which this air experienced to produce the effect of which we have just spoken, was, from the same table, 72·415. Then, to raise 46·860 grammes of air through 72°415 requiresas much heat as to raise the calorimeter or 596·8 grammes of distilled water through 1°5996; whence the specific heat of water being 1, that of the air is 0 2813.

§ 3.

*Third Method.*

Third method. The experiments we have made by following the process of which the labours of Count Rumford gave us an idea, gives us a method more simple and direct, and at the same time sufficiently exact to determine the porportion of the specific heat of water to that of air.

In these experiments the calorimeter contained as much heat

heat as 620·8 grammes of distilled water. Now, it results from the experiments made on the air, and presented in the second table, that 83·20 litres of air, or 108 32 grammes, in experiencing a lowering of its temperature of 85°, raises the temperature of the calorimeter or 620·8 grammes of distilled water at 4° : we find by this, by means of a simple calculation, that the specific heat of water being 1, that of air is 0·2697.

The determination of the specific heat of air by these three processes, leads to results very near to each other, and, taking the medium between these three, we find 0·2669. But in adopting for the proportions of the specific heat of gas to that of air, those to which we have been conducted by our first experiments, we have constructed the following table of the specific heats of the different gases.

Under the pressure of 0·76,

| | | |
|---|---|---|
| Specific heat of | water - - | 1·0000 |
| | atmospheric air | 0 2699 |
| | hydrogen gas - | 3·2936 |
| | carbonic acid - | 0·2210 |
| | oxigen - - | 0·2361 |
| | azote - - | 0·2754 |
| | olefiant gas - - | 0·4207 |
| | oxide of carbon | 0·2884 |
| | aqueous vapour | 0·8470 |

## Section IX.

### *General Considerations.*

These are the principal results to which the experiments detailed in this memoir lead. General considerations.

1st. The specific heat of gas is not the same in all, whether we have respect to the volume or to the weight.

2d. The specific heat of atmospheric air, considered with respect to its volume, increases with its density, but following a less rapid progression. Consequently, considered with re-

spect to its mass, it diminishes in proportion as the density increases, but yet pursuing a less rapid progression.

The absorption or the disengagement of caloric, which is disengaged when the air is dilated or compressed, has been attributed to a change which was supposed to operate on the specific heat of the air; but that explanation rested on a simple supposition, which our experiments, if they are exact, have now changed into a certainty.

3d. For equal volumes, the specific heat of the gases is almost null with respect to that of bodies solid or liquid.

4th. The little specific heat of the air induces us to think, that it might be of advantage, with respect to the economy of combustibles, to make use of machines in which we might employ the dilatation of air instead of that of water reduced to vapour; and the more we raised the temperature of the air, the more advantageous it would be to make use of it.

5th. We have discovered that aqueous vapour has a specific heat less than that of water. This extraordinary result has been furnished by an experiment very delicate and very difficult to make. Whence, notwithstanding all the attention we paid to it, we dare not affirm it to be correct. It will shew how highly interesting it would be to make more numerous experiments on these subjects.

6th. Again, it appears from our experiments, that we cannot admit the relation which some philosophers have thought they perceived between the specific heat of the component parts, and of the body which they form. We know that Dr. Irwin presented a theory, in which he sought to explain the heat disengaged in the combination of two bodies by the less specific heat of the composed body. But from our experiments, water presents an objection against this theory, which appears to us to be impossible to refute. In fact, we find, from the table which we have inserted, that a mixture of oxigen and hydrogen, in a proper proportion to form water, has a specific heat represented by 0·63, while the specific heat of the water formed by this mixture, would be 1. Besides, we know the enormous quantity of heat which is disengaged in the combination

bination of oxigen with hydrogen. This extraordinary, and, at the same time, very important, result, cannot be rejected without carrying the influence of these errors, which we may possibly have committed in our determinations, beyond the limits which it is reasonable to assign them.

If, on the other hand, we pay attention to the small specific heat of oxigen gas, we shall perceive, that it becomes very difficult to explain, by a change of specific heat, the heat which is disengaged in combustions in general.

We are, however, far from asserting, that there does not exist any connection between the specific heat of a compound, and those parts of which it is compounded; this is attested by many facts. In truth, we have seen, that hydrogen is of all bodies that which has the greatest specific heat; and also that the compound it forms has a specific heat much greater than that of other bodies. From hence proceeds the great specific heat of water, of vegetable and animal substances, of ammoniac, of olefiant gas, &c.

*** *The gazometer used by Messrs. Delaroche and Berard is not Dr. Wollaston's, but appears to have been that employed by Messrs. Girard in their hydrostatic lamp.—Note of Dr Wollaston.*

| Names of the Gases submitted to the Experiments. | | Number of litres of gas which passed the Calorimeter in ten minutes. | Pressure and temperature at which the current was measured. Thermometer. | Pressure and temperature at which the current was measured. Barometer. | The same number of litres reduced to the temperature of 0°. | Temperature at which the current of gas entered the calorimeter. | Temperature of the calorimeter when the current had rendered it stationary, and consequently the temperature of the gas which issued from the calorimeter. | Temperature of the air which surrounded the calorimeter. | Degrees of the thermometer lost by the current of gas in passing through the calorimeter. | Excess of the stationary temperature of the calorimeter above that of the ambient air less 2°·5, which was the effect of the tube in which the current was heated. | Excess of the temperature at which the current of gas would have maintained the calorimeter, if the current had been the same as in the first experiment on the atmospheric air, and if it had experienced the same depression of temperature. | Excess of the temperature at which the current would have sustained the calorimeter, if the circumstances had been the same as in the preceding column, and if the gas had also been subject to the pressure of 0^m 76. | Means. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | ° | me. | | | ° | | | ° | | ° | |
| Atmospheric Air. | 1st Exper. | 36·91 | 6·8 | 0·7405 | 35·99 | 97·6 | 25·183 | 7 262 | 72·415 | 15·423 | 15 423 | 15·644 | 15·734 |
| | 2d Ditto. | 30·58 | 11·2 | 0·7548 | 29 30 | 97·3 | 25 067 | 9·776 | 72·233 | 12·791 | 15 753 | 15 824 | |
| Hydrogen Gas. | 1st Ditto. | 37·84 | 8·9 | 0·7494 | 36 62 | 95·8 | 24 765 | 8·040 | 71·033 | 14·225 | 14·254 | 74·388 | 14.214 |
| | 2d Ditto. | 31·50 | 12·1 | 0·7545 | 30·14 | 95·7 | 25·807 | 12 017 | 69·893 | 11·290 | 13·972 | 14·040 | |
| Carbo. acid Gas. | 1st Ditto. | 36·11 | 7·2 | 0 7582 | 35·16 | 96·8 | 27·548 | 6·552 | 69·278 | 18·496 | 19 791 | 19·798 | 19·799 |
| | 2d Ditto. | 30·95 | 12·2 | 0·7520 | 29·60 | 97·15 | 29 598 | 12·016 | 67·552 | 15·082 | 19 662 | 19 301 | |
| Oxigen Gas. | 1st Ditto. | 37·42 | 9·0 | 0·7484 | 36·20 | 97 71 | 25·769 | 8·158 | 71·941 | 15 111 | 15 124 | 15·281 | 15·365 |
| | 2d Ditto. | 31·30 | 12·6 | 0·7503 | 29·89 | 96·30 | 27·764 | 13 205 | 68 536 | 12 059 | 15 344 | 15 449 | |
| Gas of Oxide of Azote. | | 30·31 | 9·0 | 0 7582 | 29·32 | 97·00 | 27·924 | 8·923 | 69 076 | 16 501 | 21·235 | 21·246 | 21·246 |
| Olefiant Gas. | | 30 85 | 10.0 | 0 7500 | 29 74 | 97·35 | 30·288 | 9 258 | 67 062 | 18·530 | 24 220 | 24·435 | 24·435 |
| Gas of oxide of Carbon. | | 30·85 | 9 0 | 0 7585 | 29·84 | 97·55 | 24·505 | 8·475 | 73 045 | 13·530 | 16 177 | 16 270 | 16 270 |

| Names of the gases submitted to the experiments. | | Number of litres of gas which passed the calorimeter. | Temperature and pressure by which the current was measured. Thermometer. | Barometer. | The same number of litres reduced to the temperature of °. | Temperature of the gas at its entrance into the calorimeter. | Temperature of the gas on its quitting the calorimeter, equal to the ambient air. | Degrees of heat lost by the gas in passing through the calorimeter. | Effect produced by the current of gas on the calorimeter, making allowance for the heat communicated by the tube, used to heat the gas. | Effect which the gas would have produced on the calorimeter, in the same circumstances as in the preceding column, and if it had also been submitted to the pressure of 0°·76. | Number of litres of gas (at 0°. and 0m·76) necessary to raise the temperature of the calorimeter to 4°. if this gas, in passing it had lost 85°. | Means. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Atmospheric Air. | 1st. Exper. | 81·4 | 11·0 | 0·7580 | 78·17 | 97·3 | 11·638 | 85·662 | 3·705 | 3·705 | 85·04 | |
| | | 40·2 | 11·0 | 0·7580 | 38·61 | 97·3 | 11·638 | 85·662 | 1·852 | 1·852 | 84·24 | |
| | 2d Ditto. | 79·9 | 11·0 | 0·7580 | 76·73 | 97·3 | 11·638 | 85·662 | 3·705 | 3·705 | 83·48 | 83·20 |
| | | 38·2 | 11·0 | 0·7580 | 36·69 | 97·3 | 11·638 | 85·662 | 1·852 | 1·852 | 80·04 | |
| Hydrogen Gas. | 1st Ditto. | 89·6 | 12·5 | 0·7580 | 86·04 | 95·5 | 12·443 | 83·057 | 3·610 | 3·610 | 93·16 | |
| | | 44·7 | 12·5 | 0·7580 | 42·93 | 95·5 | 12·443 | 83·057 | 1·805 | 1·805 | 92·96 | |
| | 2d Ditto. | 90·0 | 12·5 | 0·7580 | 86·43 | 95·5 | 12·443 | 83·057 | 3·610 | 3·610 | 93·60 | 93·12 |
| | | 44·6 | 12·5 | 0·7580 | 42·83 | 95·5 | 12·443 | 83·057 | 1·805 | 1·805 | 92·76 | |
| Carbonic acid Gas. | 1st. Ditto. | 64·7 | 10·8 | 0·7580 | 62·14 | 97·15 | 10·323 | 86·827 | 3·970 | 3·970 | 63·96 | |
| | | 32·3 | 10·8 | 0·7580 | 31·02 | 97·15 | 10·323 | 86·827 | 1·985 | 1·985 | 63·88 | |
| | 2d Ditto. | 63·7 | 10·8 | 0·7580 | 61·18 | 97·15 | 10·323 | 86·827 | 3·970 | 3·970 | 62·96 | 63·42 |
| | | 31·8 | 10·8 | 0·7580 | 30·54 | 97·15 | 10·323 | 86·827 | 1·985 | 1·985 | 62·88 | |
| Oxigen Gas. | 1st Ditto. | 82·9 | 12·6 | 0·7503 | 79·16 | 96·3 | 12·667 | 83·633 | 3·646 | 3·671 | 84·48 | |
| | | 41·3 | 12·6 | 0·7503 | 39·44 | 96·3 | 12·667 | 83·633 | 1·823 | 1·835 | 84·60 | |
| | 2d Ditto. | 84·0 | 12·6 | 0·7503 | 80·21 | 96·3 | 12·667 | 83·633 | 3·646 | 3·671 | 86·00 | 85·35 |
| | | 41·95 | 12·6 | 0·7503 | 40·06 | 96·3 | 12·667 | 83·633 | 1·823 | 1·835 | 85·92 | |
| Gas, oxide of Azote. | 1st Ditto. | 59·6 | 9·0 | 0·7582 | 57·66 | 97·0 | 8·865 | 88·135 | 3·800 | 3·800 | 62·92 | |
| | 2d Ditto. | 59·7 | 9·0 | 0·7582 | 57·75 | 97·0 | 8·865 | 81·135 | 3·800 | 3·800 | 63·04 | 63·24 |
| | | 30·2 | 9·0 | 0·7582 | 29·22 | 97·0 | 8·865 | 88·135 | 1·900 | 1·900 | 63·76 | |
| Olefiant Gas. | 1st Ditto. | 49·8 | 10·0 | 0·7500 | 48·00 | 97·35 | 9·969 | 87·881 | 3·886 | 3·920 | 50·64 | |
| | | 23·8 | 10·0 | 0·7500 | 23·00 | 97·35 | 9·469 | 87·881 | 1·942 | 1·960 | 48·40 | |
| | 2d Ditto. | 50·0 | 10·0 | 0·7500 | 48·19 | 97·35 | 9·469 | 87·881 | 3·886 | 3·920 | 58·84 | 49·52 |
| | | 23·7 | 10·0 | 0·7500 | 22·84 | 97·35 | 9·469 | 87·881 | 1·943 | 1·960 | 48·20 | |
| Gas, oxide of Carbon. | 1st Ditto. | 77·6 | 9·0 | 0·7535 | 75·07 | 97·55 | 8·475 | 89·080 | 3·703 | 3·726 | 84·48 | |
| | | 38·2 | 9·0 | 0·7535 | 36·95 | 97·55 | 8·475 | 89·080 | 1·852 | 1·863 | 88·16 | |
| | 2d Ditto. | 78·0 | 9·0 | 0·7535 | 75·45 | 97·55 | 8·475 | 89·080 | 3·705 | 3·726 | 84·88 | 84·62 |
| | | 39·5 | 9·0 | 0·7535 | 38·21 | 97·55 | 8·475 | 89·080 | 1·852 | 1·863 | 85·96 | |

## V.

### *Experiments in Electricity. By Mr.* T. HOWLDY.

*To Mr. Nicholson.*

SIR,

IN consequence of repeating and varying an experiment described in Cavallo's Complete Treatise on Electricity*, I discovered, about four years ago, the following experiments, which, I have reason to think, are by no means generally known to electricians, and therefore an account of them may possibly deserve to be recorded in your excellent Journal.

The apparatus employed in performing them were simply two pith-balls and a narrow piece of wood eight inches long, having in the middle of one of its surfaces a small groove extending its whole length. The pith-balls were three-eighths of an inch in diameter, and the groove was fitted, as nearly as it could be, to their curvature, its depth being half the diameter of the balls. After a jar, containing about 168 square inches of coated surface had been placed at the prime conductor of the electrical machine, and one extremity of the universal discharger had been connected with its outside, the piece of wood was insulated upon the table of that instrument, the extremities of the opposite wires of which, without their balls, were placed at $4\frac{1}{2}$ inches distance from each other in the groove; the wires being so adjusted as to lie nearly parallel with its bottom, but without touching that or its sides. The charge communicated to the jar was not, in any case, so strong as to be capable of exploding over the interval in the circuit; but, on the contrary, was always much weaker; and though this observation does not define its strength with precision, yet it will enable the electrician, who is desirous of repeating the experiments, to ascertain, by one or two trials, a charge which will produce the intended effects; it may not be improper to add, that the mode of discharging the jar was by bringing one

* Vol. I. p. 259, Fourth Edition.

ball

ball of an insulated discharger *quickly into contact* with the ball of the jar, and then *instantly* withdrawing it. In describing the experiments, I beg leave for the sake of conciseness, to call the wire communicating with the outside of the jar the negative, and that communicating with its inside the positive wire.

*Experiment* 1.

One of the pith balls was put in the groove nearly in contact with the extremity of the positive wire, and the jar was then charged; when the discharge was made, the pith-ball instantly ran from the positive to the negative wire, where it stopped.

*Experiment* 2.

The pith-ball was placed nearly in contact with the negative wire instead of the positive. On discharging the jar, the pith-ball ran from the negative to the positive wire, where it stopped. In this instance the ball moved in a direction exactly the contrary to that in which it moved in the first experiment.

*Experiment* 3.

The pith-ball was placed exactly in the middle between the extremities of the two wires, which still remained at the same distance.

When the discharge was made, the pith ball kept its place, and seemed not to be at all affected by it.

*Experimet* 4.

One pith-ball was placed, as in experiment 1, at the positive, and another, as in experiment 2, at the negative wire. On discharging the jar, the ball at the negative wire ran towards the positive, and that at the positive ran towards the negative; the balls met exactly in the middle of the interval, and then stopped.

This experiment affords a pleasing and striking instance of two

two bodies moving simultaneously in opposite directions in the electrical circuit.

*Experiment 5.*

The two pith-balls were put exactly in the middle of the interval, as the single pith-ball was in experiment 3. When the jar was discharged, no perceptible motion was observed in either of the pith-balls.

The preceding experiments at the time of their discovery, were repeatedly performed in order to verify the results; and the interval in the circuit has since been varied from two to six inches in length; experiments 1 and 2 have been made even with an interval of seven inches; and, in every instance, the results have been uniformly the same as above stated, when the strength of the charge has been proportioned to the increased or diminished distance forming the interval in the circuit.

With respect to the cause or causes producing the movements of the pith-balls in experiments 1, 2, and 4, I shall, at present, offer no opinion; it is obvious, however, to observe, that if these experiments had been known either to the electricians of a former period, who devised and supported, or to those of the present period, who have adopted and defended, the hypothesis of two fluids moving in opposite directions during the discharge of the Leyden jar; that they would have eagerly brought them forward as strongly favouring that hypothesis; and the remarkable circumstance, that the balls in experiments 3 and 5 are not affected by the discharge, would undoubtedly have been considered as giving additional probability to the hypothesis, if not confirming its truth.

Having satisfied myself with regard to the certainty and constancy of the effects produced in the foregoing experiments, I wished to ascertain what effects would ensue when the charge of the jar was sufficiently powerful to pass over the interval in circuit with an explosion: they were found to be as follow:

When a single pith-ball was placed either at the positive

or

negative wire; or in the interval between both; it was always driven by the discharge nearly perpendicularly upwards to the height of twenty or thirty inches; and sometimes descended at a very small, and sometimes at a considerable distance, from whence it was projected. When two pith-balls are placed in contact with each other in any part of the interval, they are likewise driven upwards, and often separated by the discharge, sometimes falling at a small distance from each other, and at other times at a greater distance on opposite sides of the table.

I am,

Sir,

Your obliged Servant,

THOMAS HOWLDY.

*Hereford,*
*October* 16*th*, 1813.

---

## VI.

*On the elementary Particles of certain Crystals: By* WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S. From the Philosophical Transactions for* 1813.

Whether the regular octohedron or tetrahedron be the primitive.

AMONG the known forms of crystallized bodies, there is no one common to a greater number of substances than the regular octohedron, and no one in which a corresponding difficulty has occurred with regard to determining which modification of its form is to be considered as primitive; since, in all these substances, the tetrahedron appears to have equal claim to be received as the original from which all their other modifications are to be derived.

The relations of these solids to each other is most distinctly exhibited to those who are not much conversant with crystallography, by assuming the tetrahedron as primitive, for this may immediately be converted into an octohedron by the removal of four smaller tetrahedrons from its solid angles. (Fig. 1.)

The

Fluor spar the most convenient for experiments of fracture.

The substance which most readily admits of division by fracture into these forms is fluor spar; and there is no difficulty in obtaining a sufficient quantity for such experiments. But it is not, in fact, either the tetrahedron or the octohedron, which first presents itself as the apparent primitive form obtained by fracture.

If we form a plate of uniform thickness by two successive divisions of the spar, parallel to each other, we shall find the plate divisible into prismatic rods, the section of which is a rhomb of 72° 32′ and 109° 28′ nearly; and if we again split these rods transversely, we shall obtain a number of regular acute rhomboids, all similar to each other, having their superficial angles 60° and 120°, and presenting an appearance of primitive molecule, from which all the other modifications of such crystals might very simply be derived. And we find, moreover, that the whole mass of fluor might be divided into, and conceived to consist of, these acute rhomboids alone, which may be put together so as to fit each other without any intervening vacuity.

But, since the solid thus obtained (as represented fig. 2.) may be again split by natural fractures at right angles to its axis (fig. 3.) so that a regular tetrahedron may be detached from each extremity, while the remaining portion assumes the form of a regular octohedron; and, since every rhomboid that can be obtained, must admit of the same division into one octohedron and two tetrahedrons, the rhomboid can no longer be regarded as the primitive form; and since the parts into which it is divisible are dissimilar, we are left in doubt which of them is to have precedence as primitive.

Neither the octohedron nor tetrahedron can be arranged in stable equilibrium,

In the examination of this question, whether we adopt the octohedron or the tetrahedron as the primitive form, since neither of them can fill space without leaving vacuities, there is a difficulty in conceiving any arrangement in which the particles will remain at rest: for whether we suppose, with the Abbé Haüy, that the particles are tetrahedral with octohedral cavities, or, on the contrary, octohedral particles regularly arranged with tetrahedral cavities, in each case the mutual contact

contact of adjacent particles is only at their edges; and although in such an arrangement it must be admitted that there may be an equilibrium, it is evidently unstable, and ill-adapted to form the basis of any permanent crystal.

Explanation by which this difficulty is obviated.

More than three years have now elapsed since a very simple explanation of this difficulty occurred to me. As in the course of that time I had not discovered it to be liable to any crystallographical objection, and as it had appeared satisfactory to various mathematical and philosophical friends to whom I proposed it, I had engaged to make this the subject of the Bakerian lecture of the present year, hoping that some further speculations, connected with the same theory, might lead to more correct notions than are at present entertained of crystallization in general.

The position was antecedently discovered by Dr. Hooke.

At the time when I made this engagement, I flattered myself that the conception might be deserving of attention from its novelty. But I have since found, that it is not altogether so new as I had then supposed it to be; for, by the kindness of a friend, I have been referred to Dr. Hooke's Micrographia, in which is contained, most clearly, one essential part of the same theory.

However, since the office of a lecturer is properly to diffuse knowledge already acquired, rather than to make known new discoveries in science, and since these hints of Dr. Hooke have been totally overlooked, from having been thrown out at a time when crystallography, as a branch of science, was wholly unknown, and consequently not applied by him to the extent which they may now admit, I have no hesitation in treating the subject as I had before designed. And when I have so done, I shall quote the passage from Dr. Hooke, to shew how exactly the views which I have taken have, to a certain extent, corresponded with his; and I shall hope that, by the assistance of such authority, they may meet with a more favourable reception.

The theory. Particles perfectly spherical, and

The theory to which I here allude is this—that, with respect to fluor spar, and such other substances, as assume the octohedral and tetrahedral forms, all difficulty is removed by supposing

brought as near as possible, will form the solids. ing the elementary particles to be perfect spheres, which, by mutual attraction, have assumed that arrangement which brings them as near to each other as possible.

Instance. One lamina disposed to break in lines at 60°. The relative position of any number of equal balls in the same plane, when gently pressed together, forming equilateral triangles with each other, (as represented perspectively in fig. 4.) is familiar to every one; and it is evident, that if balls so placed were cemented together, and the stratum thus formed were afterwards broken, the straight lines in which they would be disposed to separate would form angles of 60° with each other.

Formation of the tetrahedron; If a single ball were placed any where at rest upon the preceding stratum, it is evident that it would be in contact with three of the lower balls, (as in fig. 5,) and that the lines joining the centres of four balls so in contact, or the planes touching their surfaces, would include a regular tetrahedron, having all its sides equilateral triangles.

and the octohedron. The construction of an octohedron, by means of spheres alone, is as simple as that of the tetrahedron. For if four balls be placed in contact on the same plane in the form of a square, then a single ball resting upon them in the centre being in contact with each pair of balls, will present a triangular face rising from each side of the square, and the whole together will represent the superior apex of an octohedron; so that a sixth ball, similarly placed underneath the square, will complete the octohedral group, fig. 6.

The octohedron is the same whether began on the foundation of four or of three spheres; There is one observation with regard to these forms that will appear paradoxical, namely, that a structure which, in this case, was begun upon a square foundation, is really intrinsically the same as that which is begun upon the triangular basis. But if we lay the octohedral group, which consists of six balls, on one of its triangular sides, and consequently with an opposite triangular face uppermost, the two groups, consisting of three balls each, are then situated precisely as they would be found in two adjacent strata of the triangular arrangement.

and is again convertible to a tetrahedron. Hence, in this position we may readily convert the octohedron into a regular tetrahedron, by addition of four more balls

balls (fig. 7.) One placed on the top of the three that are uppermost, forms the apex; and if the triangular base on which it rests be enlarged by addition of three more balls regularly disposed around it, the entire group of ten balls will then be found to represent a regular tetrahedron.

The acute rhomboid.

For the purpose of representing the acute rhomboid, two balls must be applied at opposite sides of the smallest octohedral group, as in fig. 9. And if a greater number of balls be placed together, fig. 10 and 11, in the same form, then a complete tetrahedral group may be removed from each extremity, leaving a central octohedron, as may be seen in fig. 11, which corresponds to fig. 3.

The passage of Dr. Hooke, from which I shall quote so much as to connect the sense, is to be found at page 85 of his Micrographia.

Passage from Dr. Hooke, containing this theory.

"From this I shall proceed to a second considerable phenomenon, which these diamants (meaning thereby quartz crystals) exhibit, and that is the regularity of their figure. This I take to proceed from the most simple principle that any kind of form can come from, next the globular; for—I think I could make probable, that all these regular figures arise only from three or four several positions or postures of globular particles, and those the most plain and obvious, and necessary conjunctions of such figured particles that are possible. And this I have *ad oculum* demonstrated with a company of bullets, so that there was not any regular figure which I have hitherto met withal of any of those bodies that I have above named that I could not, with the composition of bullets or globules, imitate almost by shaking them together.

"Thus, for instance, we find that globular bullets will, of themselves, if put on an inclining plane, so that they may run together, naturally run into a triangular order composing all the variety of figures that can be imagined out of equilateral triangles, and such you will find upon trial all the surfaces of alum to be composed of.

"Nor does it hold only in superficies, but in solidity also; for it is obvious, that a fourth globule laid upon the third in

this

this texture composes a regular tetrahedron, which is a very usual figure of the crystals of alum. And there is no one figure into which alum is observed to be crystallized, but may, by this texture of globules, be imitated, and by no other."

It does not appear in what manner this most ingenious philosopher thought of applying this doctrine to the formation of quartz crystal, of vitriol, of salt-petre, &c. which he names. This remains among the many hints which the peculiar jealousy of his temper left unintelligible at the time they were written, and which, notwithstanding his indefatigable industry, were subsequently lost to the public, for want of being fully developed.

We have seen, that by due application of spheres to each other, all the most simple forms of one species of crystal will be produced, and it is needless to pursue any other modifications of the same form, which must result from a series of decrements produced according to known laws.

Union of other solids allied to the sphere.

Since, then, the simplest arrangement of the most simple solid that can be imagined, affords so complete a solution of one of the most difficult questions in crystallography, we are naturally led to inquire what forms would probably occur from the union of other solids most nearly allied to the sphere. And it will appear that by the supposition of elementary particles that are spheroidical, we may frame conjectures as to the origin of other angular solids well known to crystallographers.

*The Obtuse Rhomboid.*

Crystallographic solids formed by oblate spheroids The obtuse rhomboid.

If we suppose the axis of our elementary spheroid to be its shortest dimension, a class of solids will be formed which are numerous in crystallography. It has been remarked above, that by the natural grouping of spherical particles, fig. 10, one resulting solid is an acute rhomboid, similar to that of fig. 2, having certain determinate angles, and its greatest dimension in the direction of its axis. Now, if other particles having the same relative arrangement be supposed to have the form of oblate spheroids, the resulting solid, fig. 12, will still

be

be a regular rhomboid; but the measures of its angles will be different from those of the former, and will be more or less obtuse, according to the degree of oblateness of the primitive spheroid.

It is, at least, possible, that carbonate of lime and other substances, of which the forms are derived from regular rhomboids as their primitive form, may, in fact, consist of oblate spheroids as elementary particles.

As possibly in carbonate of lime;

It deserves to be remarked, that the conjecture to which we are thus led by a natural transition, from consideration of the most simple form of crystals, was long since entertained by Huyghens*, when treating of the oblique refraction of Iceland spar, which he so skilfully analysed. The peculiar law observable in the refraction of light by that crystal, he found might be explained on a supposition of spheroidical undulations propagated through the substance of the spar, and these, he thought, might, perhaps, be owing to a spheroidical form of its particles, to which the disposition to split into the rhomboidal form might also be ascribed.

and conjectured by Huyghens.

By some oversight, however, the proportion of the axes of such an elementary spheroid is erroneously stated to be 1 to 8; but this is probably an error of the press, instead of 1 to 2,8, for I find the proportion to be nearly 1 to 2·87. In fig. 15, F is the apex of a tetrahedron cut from an acute rhomboid similar to fluor spar, and the sections of two spheres are represented round the centres F and C. I is the apex of a corresponding portion cut from the summit of a rhomboid of Iceland spar, as composed of spheroids having the same diameter as the spheres. In the former, the inclination FCT of the edge of the tetrahedron to its base is 54° 44′; in the latter, the inclination ICT is 26° 15′; and the altitudes FT, IT, are as the tangents of these angles 1414 to 493 : : 2·87 : 1, which also expresses the ratio of the axis of the sphere to that of the spheroid, or the proportional diameters of the generating ellipse.

Numerical dimensions.

* Huyghenii Op. Reliq. Tom. 1. Tract de Lumine, p. 70.

*Hex-*

### *Hexagonal Prisms.*

Oblong spheroids would give hexagonal prisms.

If our elementary spheroid be, on the contrary, oblong, instead of oblate, it is evident, that by mutual attraction, their centres will approach nearest to each other when their axes are parallel, and their shortest diameters in the same plane (fig. 13.) The manifest consequence of this structure would be, that a solid so formed would be liable to split into plates at right angles to the axes, and the plates would divide into prisms of three or six sides, with all their angles equal, as occurs in phosphate of lime, beryl, &c.

It may further be observed, that the proportion of the height to the base of such a prism must depend on the ratio between the axes of the elementary spheroid.

### *The Cube.*

The cube cannot be formed by spheres in contact;

Although I could not expect that the sole supposition of spherical or spheroidical particles would explain the origin of all the forms observable among the more complicated crystals, still the hypothesis would have appeared defective if it did not include some view of the mode in which so simple a form as the cube may originate.

A cube may evidently be put together of spherical particles arranged four and four above each other; but we have already seen, that this is not the form which simple spheres are naturally disposed to assume, and consequently this hypothesis *alone* is not adequate to its explanation, as Dr. Hooke had conceived.

nor by oblate spheroids.

Another obvious supposition is, that the cube might be considered as a right-angled rhomboid, resulting from the union of eight spheroids having a certain degree of oblateness (2 to 1) from which a rectangular form might be derived. But the cube so formed would not have the properties of the crystallographical cube. It is obvious that, though all its diagonals would thus be equal, yet one axis parallel to that of the elementary spheroid would probably have properties different from

from the rest. The modifications of its crystalline form would probably not be alike in all directions as in the usual modifications of the cube, but would be liable to elongation in the direction of its original axis. And if such a crystal were electric, it would have but one pair of poles instead of having four pair, as in the crystals of boracite.

There is, however, an hypothesis, which at least has simplicity to recommend it; and if it be not a just representation of the fact, it must be allowed to bear a happy resemblance to truth.

Two sets of four spheres each, all equal (but under condition that no two adjacent spheres of different sets should be more distant than any other two) would form a cube.

Let a mass of matter be supposed to consist of spherical particles all of the same size, but of two different kinds, in equal numbers, represented by black and white balls; and let it be required, that in their perfect intermixture every black ball shall be equally distant from all surrounding white balls, and that all adjacent balls of the same denomination shall also be equidistant from each other. I say, then, that these conditions will be fulfilled if the arrangement be cubical, and that the particles will be in equilibrio. Fig. 14 represents a cube so constituted of balls, alternately black and white throughout. The four black balls are all in view. The distances of their centres being every way a superficial diagonal of the cube, they are equidistant, and their configuration represents a regular tetrahedron; and the same is the relative situation of the four white balls. The distances of dissimilar adjacent balls are likewise evidently equal; so that the conditions of their union are complete, as far as appears in the small group; and this is a correct representative of the entire mass that would be composed of equal and similar cubes.

Properties of boracite alluded to.

Since the crystalline form and electric qualities of boracite are perhaps unique, any explanation of properties so peculiar can hardly be expected. It may, however, be remarked, that a possible origin of its four pair of poles may be traced in the structure here represented; for it will be seen, that a white ball and a black one are regularly opposed to each othe at the extremities of each axis of the cube.

This hypoth...

An hypothesis of uniform intermixture of particle wit par- ticle,

sis does not require physical atoms, but only spheres of energy.

ticle, accords so well with the most recent views of binary combination in chemistry, that there can be no necessity, on the present occasion, to enter into any defence of that doctrine, as applied to this subject. And though the existence of ultimate physical atoms, absolutely indivisible, may require demonstration, their existence is by no means necessary to any hypothesis here advanced, which requires merely mathematical points endued with powers of attraction and repulsion equally on all sides, so that their extent is *virtually* spherical; for, from the union of such particles the same solids will result as from the combination of spheres impenetrably hard.

The metals, which are probably the most simple bodies, favour this doctrine.

There remains one observation with regard to the spherical form of elementary particles, whether actual or virtual, that must be regarded as favourable to the foregoing hypothesis, namely, that many of those substances which we have most reason to think simple bodies, as among the class of metals, exhibit this further evidence of their simple nature, that they crystallize in the octohedral form, as they would do if their particles were spherical.

But it must, on the contrary, be acknowledged, that we can, at present, assign no reason why the same appearance of simplicity should take place in fluor spar, which is presumed to contain at least two elements; and it is evident, that any attempts to trace a general correspondence between the crystallographical and supposed chemical elements of bodies must, in the present state of these sciences, be premature.

---

*Note.*

Theory of M. Prechtl of the compression of soft spheres.

A theory has lately been advanced* by M. Prechtl, which attempts to account for various crystalline forms from the different degrees of compression that soft spheres may be supposed to undergo in assuming the solid state. It is supposed,

* Journal des Mines, No. 166.

that

that with a certain degree of softness, and of relative attraction, the particles will be surrounded each by four others, and will all be tetrahedral, although, in fact, it be demonstrably impossible that tetrahedrons alone should fill any space.

It is next supposed, that soft spheres less compressed will be surrounded by five others, and will be formed into triangular prisms, comprised under five similar and equal planes. That they should be similar is impossible, and it is further demonstrable, that when the triangular termination of such a prism is equal in area to each rectangular side of the prism, so as to present equal resistance, according to the hypothesis, then the triangular faces will be nearer to the centre in the proportion of three to four, so that the attractions will not be equal as the hypothesis would require. Objections.

A third hypothesis of M. Prechtl is, that the degree of compressibility may be such that each particle will be surrounded by six others, giving it the form of a cube, which, it must be admitted, is a very possible supposition.

All further application of the same hypothesis is precluded by M. Prechtl, by denying that one particle can be surrounded by more than six others; although, in fact, it is most evident, that any sphere, when not compressed, will be surrounded by twice that number, and consequently by a slight degree of compression will be converted into a dodecahedron, according to the most probable hypothesis of simple compression.

---

*Annotation.*—W. N.

Dr. George Fordyce, at the end of his pamphlet entitled "Elements of Agriculture and Vegetation," published in London, 1771, has given three plates to show the combination of bodies by the apposition of the spheres which surround the particles where the powers of attraction and repulsion are in equilibrio. He considers the chemical union of two particles as producing a compound endued with a new sphere of action; as,

as, for example, volatile alkali and acid having formed sal-ammoniac, becomes a distinct spherical element in his figure, and affords a new compound with copper.

I would not venture to depart from the wise reserve of the learned author of this memoir, by speculating upon the crystalline forms capable of being produced in combinations of elements subordinate to each other in simplicity, according to Dr. Fordyce's notion, which appears to agree with the processes of nature; nor by considering the variations which might be assumed, or perhaps deduced from the relative magnitudes, densities, definite proportions in mass and electric polarities (if such can be supposed in simple particles of bodies.) I would only hint, that, though an attempt to establish these doctrines would undoubtedly be premature, it may, nevertheless, be expected, that considerable advantages would be derived from their organization and arrangement.

---

## VII.

*Observations relative to the near and distant Sight of different Persons. By* JAMES WARE, *Esq. F. R. S. From the Philosophical Transactions for* 1813.

Commencement of near-sight is early; of distant sight late.

THE fact that near-sightedness most commonly commences at an early period of life, and distant-sightedness generally at an advanced age, is universally admitted. Exceptions, however, to these rules, so frequently occur, that I flatter myself a brief statement of some of the coincident circumstances attendant on these different imperfections in vision, may not be found wholly undeserving the attention of the Royal Society. Near-sightedness usually comes on between the ages of ten and eighteen. The discovery of it most commonly arises from accident; and, at first, the inconvenience it occasions is so little, that it is not improbable the imperfection would remain altogether unnoticed, if a comparison were not instituted with the

he sight of others, or if the experiment were not made of ooking through a concave glass. Among persons in the inerior stations of society, means are rarely resorted to for orrecting slight defects of this nature! and, indeed, I have eason to believe the imperfection in such people is not unfrequently overcome by the increased exertions that are made y the eye to distinguish distant objects. This, however, is ot the case, in the present day, with persons in the higher anks of life. When these discover that their discernment of listant objects is less quick or less correct than that of others, hough the difference may be very slight, influenced, perhaps, y fashion more than by necessity, they immediately have ecourse to a concave glass; the natural consequence of which s, that their eyes, in a short time, become so fixed in the state equiring its assistance, that the recovery of distant vision is endered afterwards extremely difficult, if not quite imposible. With regard to the proportion between the number of ear-sighted persons in the different ranks of society, I have aking pains to obtain satisfactory information, by making inquiries in those places where a large number in these several lasses are associated together. I have inquired, for instance, f the surgeons of the three regiments of foot guards, which onsist of nearly ten thousand men; and the result has been, hat near-sightedness, amongst the privates, is almost utterly nknown. Not half a dozen men have been discharged, nor alf a dozen recruits rejected, on account of this imperfection, n the space of nearly twenty years; and yet many parts of soldier's duty require him to have a tolerably correct view f distant objects; as of the movements of the fuglem n in xercise, and of the bull's eye when shooting at the target; he want of which might furnish a plausible apology for a kulker to skreen himself from duty, or to get his discharge rom the service. I pursued my inquiries at the military school t Chelsea, where there are thirteen hundred children, and I ound that the complaint of near-sightedness had never been made among them until I mentioned it; and there were then nly three who experienced the least inconvenience from it. After

The lower ranks do not use glasses for near-sight;

and they are less subject to it.

Youth in the military hospital, &c. are not affected;

After this, I inquired at several of the colleges in Oxford and Cambridge; and, though there is a great diversity in the number of students who make use of glasses in the various colleges, they are used by a considerable proportion of the whole number in both Universities; and, in one college in Oxford, I have a list of the names of not less than thirty-two out of one hundred and twenty-seven, who wore either a hand-glass or spectacles, between the years 1803 and 1807. It is not improbable, that some of these were induced to do it solely because the practice was fashionable; but, I believe, the number of such is inconsiderable, when compared with that of those whose sight received some small assistance from them, though this assistance could have been dispensed with, without inconvenience, if the practice had not been introduced. The misfortune resulting from the use of concave glasses is this, that the near-sightedness is not only fixed by it, but a habit of inquiry is induced with regard to the extreme perfection of vision; and, in consequence of this, frequent changes are made for glasses that are more and more concave, until at length the near sightedness becomes so considerable, as to be rendered seriously inconvenient and afflicting. It should be remembered, that, for common purposes, every near-sighted eye can see with nearly equal accuracy through two glasses, one of which is one number deeper than the other; and though the sight be in a slight degree more assisted by the deepest of these than by the other, yet on its being first used, the deepest number always occasions an uneasy sensation, as if the eye was strained. If, therefore, the glass that is most concave be at first employed, the eye, in a little time, will be accommodated to it, and then a glass one number deeper may be used with similar advantage to the sight; and if the wish for enjoying the most perfect vision be indulged, this glass may soon be changed for one that is a number still deeper, and so in succession, until, at length, it will be difficult to obtain a glass sufficiently concave to afford the assistance that the eye requires*.

but at the universities many.

Explanation that the use of glasses increases the evil.

* I have observed, that most of the near-sighted persons with whom

Although

Although near-sightedness is, in general, gradual in its progress, instances occasionally occur of its existence, in a considerable degree, even in children; in whom it is sometimes discovered almost as soon as they begin to take notice of the objects around them. This may be occasioned by some degree of opacity in the transparent parts of the eye; but such a cause of near-sightedness is easily discovered by an examination, and is quite different from that state of the eye to which the term myopia, or near-sightedness, is usually applied; by which is simply meant too great a convexity either in the cornea or in the crystalline, in proportion to the distance of these parts from the retina. In such cases of extreme near-sightedness in children, it is sometimes necessary to deviate from a rule which, in slighter cases, I always follow, of discouraging the use of spectacles; since, without their assistance, it would be impossible for them to prosecute their learning with ease or convenience.

Near-sightedness in children.

Extreme near-sightedness is sometimes occasioned by an evident change in the spherical figure of the cornea, and its assumption of a conical shape. This morbid state of the cornea is not only productive of near-sightedness, but when the projection is considerable, vision is so much confused, that it affords little or no service, and cannot be amended by any glass. The cornea, in most of these cases, is preternaturally thin, and not unfrequently it is accompanied with symptoms of general debility, under which last circumstance chalybeate medicines, and bracing applications to the eye, have been found to afford considerable benefit.

Case of disease producing near-sight.

Near-sightedness, to an alarming degree, has sometimes attacked young persons suddenly. A remarkable case of this kind came under my notice a few years ago, in a young gentleman

Instance of sudden and alarming near-sightedness.

I have had an opportunity of conversing, have had the right eye more near-sighted than the left; and I think it not improbable, that this difference been the two eyes has been occasioned by the habit of using a single concave hand-glass; which, being most commonly applied to the right eye, contributes, agreeably to the remark above-mentioned, to render this eye more near-sighted than the other.

tleman at Westminster school, who had been attended by Sir George Baker and Mr. Sutherland, on account of a variety of anomalous nervous symptoms. These had wholly left him before I was consulted; and the consultation with me was solely for the purpose of determining whether he might be permitted to make use of concave glasses, and to return to the business of the school. The patient's health at that time not being perfectly restored, it was thought adviseable to send him, for a few weeks, into the country, and to postpone the use of glasses. This advice was followed; but in ten days the afflicted youth died suddenly. No anatomical examination of the head was permitted by the relatives. It seems, however, probable, that the near-sightedness, as well as the previous indisposition, no less than the death of the patient, were occasioned by the pressure of a morbid substance of some kind or other, on the source of the nerves in the brain.

*(To be continued.)*

---

## SCIENTIFIC NEWS.

### *Oxides produced by Electricity.*

THE fine figures produced on paper by the explosion, and consequent oxidation of various metals with an electrical battery, cannot be effectually represented by engravings. Mr. Singer has requested me to state, that it is his intention to illustrate a few copies of his "Elements of Electricity and Electro-Chemistry," with some real specimens of the oxides, struck by his powerful apparatus. Those who desire such copies may have them secured by an early transmission of their names to Mr. Singer.

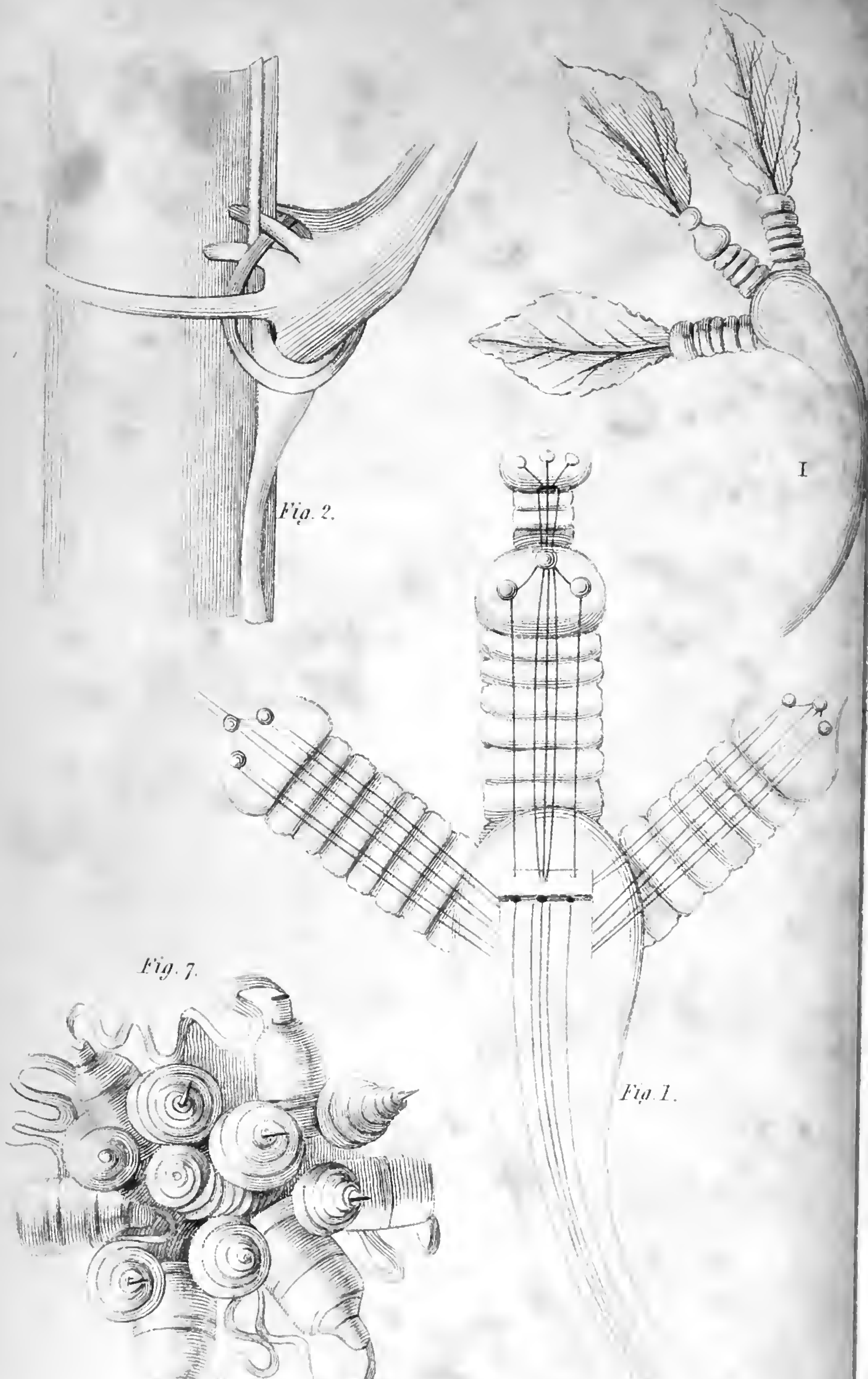


Fig. 2.
I
Fig. 1.
Fig. 7.

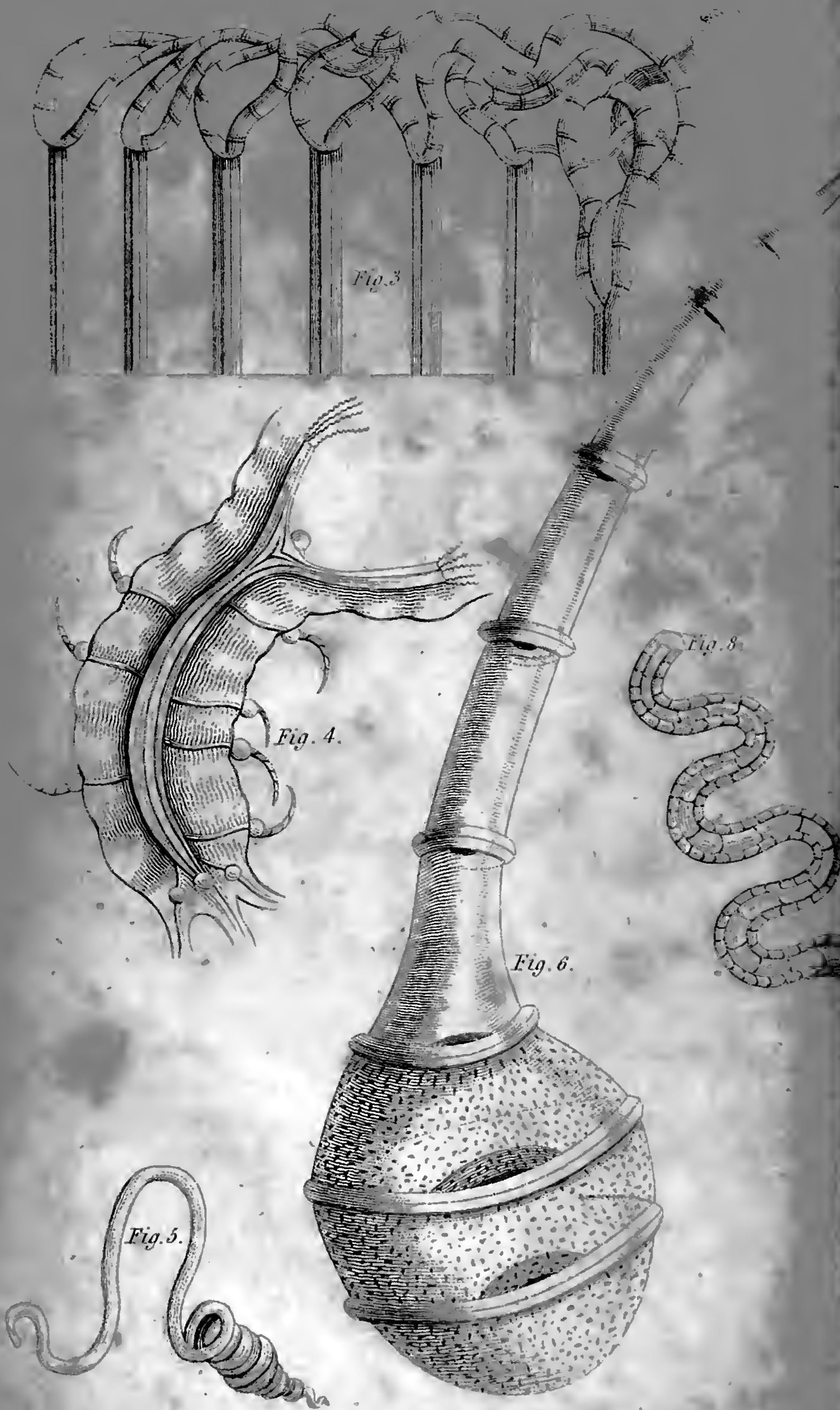


Fig.3
Fig. 4.
Fig.8.
Fig. 6.
Fig.5.

A

# JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

*DECEMBER*, 1813.

ARTICLE I.

## *On the Geological System of Werner.*

(*Concluded from p.* 162.)

IF the preceding exposition appear to have entered too much into detail, or to have assigned too great an importance to the speculative part of the subject, it must be recollected, that the names and descriptive language employed by Werner, have intervoven his speculations with his statements of facts; so that in order fully to comprehend descriptions after his manner, his theory must previously be understood. The conclusions also presented by this theory have been deduced, we are informed, by Mr. Jameson, "after *deep reflection*, and in conformity with the strictest rules of induction*:" they afford, therefore, a test of Werner's talents for such enquiries, and the consideration of them will help to determine the degree of confidence with which we may, in general, take him for our guide. Werner has blended his facts and speculations together.

In examining the Wernerian theory of the earth, it would be interesting to compare its tenets with those of Dr. Hutton, Huttonian theory.

* Jameson I. Introduction, p. xxii.

which afford the advantage of very perfect contrast, and are, indeed, the only adverse opinions which the editor of Werner's system has taken notice of. But such a comparison is foreign to the design of the present paper; and it has been already instituted, with much ability and eloquence, in a work expressly devoted to that object*. Without entering, however, into any detail of controversy, an impartial observer may be permitted to remark, that each party on this instructive question, seems to have been much more successful in proving the weakness of the opposite cause, than in establishing their own.

Extreme difficulty of accounting for the retreat of the waters.

But to some of the fundamental principles of Werner there are very obvious objections, which appear to be insuperable. The evidence, it is true, for the former submersion of the globe, and even for the repeated submersion of many parts of it, is such as cannot be resisted. But waving the objections to the solubility of all the solid matter of the earth in water, it seems almost impossible that the dissolving fluid could have disappeared, according to Werner's hypothesis, by "gradual diminution" only, without any movement of the land. It has been stated by Mr. Playfair, &c.* that, in order to the disclosure of the parts of the surface now elevated 10,000 feet above the sea by a retirement of the investing water only, a bulk of fluid must have been removed equal to more than a seven hundredth part of the whole magnitude of the globe; and if, as Werner supposes, the *whole* earth was submerged at the same time, the depth of fluid must be increased to more than double that now stated, and its bulk in a much greater proportion. Where, then, are we to find a place of retirement for so vast a quantity of matter? Not in central caverns‡; for

* "A comparative view of the Huttonian and Neptunian Systems of Geology, in answer to the illustrations of the Huttonian Theory of the Earth."—Edinburgh, 1802.

† Illustrations of the Huttonian Theory, § 37.

‡ This was the opinion of De Luc and De Lametherie.—See Jameson, III, p.76.

these

these are positively disallowed by Werner; and the mean density of the earth will not admit of their existence. There is truly no place of retreat for all this water, if it retained its liquid form; and it can have disappeared only by means of some permanent change in its condition; by being converted into vapour, or resolved into its elements, or consolidated in some of the denser material of the globe, conclusions, any of which, if they be not evidently absurd, encroach on probability too largely to be admitted without direct proof—yet these or other premises, equally remote from demonstration, are essentially connected with Werner's view of this question.

There is no place of retreat if liquid; and if solidified or vaporized, this would be pure supposition;

The force of this difficulty is very much increased by the existence of what are called the *overlying formations*; for they imply, according to Werner, repeated risings and depressions of the dissolving fluid. Let us take only the most remarkable of these, the *newest Flœtz-trap* rocks. For their production, it is stated, that, after a vast extent of land had been disclosed, the waters rose again to the height at least at which these rocks are found. But at the Pic of Teneriffe, there is basalt at 11,000 feet above the present level of the sea, and Humboldt found that rock so greatly elevated as 14,700 feet at Pinchinea, near the city of Quito*; and for every foot which the fluid sunk or rose, at either of those places, a shell of corresponding depth must have been moved over all the surface of the globe. Even supposing the disappearance of such a quantity of matter to have been accounted for, how was it again produced? Was it condensed a second time from vapour or from gas, or regenerated from the solid form which, otherwise, we must suppose it to have assumed?

and inconsistent with the repeated risings, &c.

But a single return of the waters will not be enough for Werner—for several are supposed to have taken place. That which gave rise to the overlying primitive rocks is one of them, and the newest Flœtz-trap formation will furnish many more. From what has been quoted already, it appears that the rapid rise, the calming, and deposition during the settle-

Difficulty more fully stated.

* Tableau Physique, &c. p. 123.

ment of the fluid, which constituted that formation, have been inferred from its exhibiting a succession, from coarse fragments through sand-clay and wacke, to basalt. But this succession is sometimes frequently repeated—the hill of Weissenstein in Hessia, for example*, exhibits at least three such repetitions. The conclusions deducible from these appearances in one instance, must equally result from them in all; and however small the change of level at each alternation, we shall have to account, in every case, for a corresponding change throughout the globe; unless we most gratuitously suppose all this to have been effected by internal commotion of the fluid, while its surface remained unmoved.

Censure on the liberty of speculating.

Surely the remark of D'Aubuisson upon one of the volcanic hypotheses of Dolomieu, falls with more than equal weight upon this part of Werner's speculations. "Je crois que le tems est venu, au il faut retirer au geologues le droit qu'ils se sont arrogè de commander a la nature, de lui imprimer a leur gre, les mouvements les plus extraordinaires; de faire sortir selon qu'il les jugent apropos, les mers de leurs limites, de les faire aller, venir, de les transporter là ou bon leur semble; et le tout pour expliquer de pures hypotheses†."

Werner's assumption of the immobility of the land appears less probable than the contrary position.

The assumption however, of the immobility of the land, from whence these difficulties arise, is by no means necessary for the remaining purposes of Werner; for it is plain, that the same effects would have resulted from the protrusion of the solid matter, as from the subsidence of the surrounding fluid. The evidence, too, in favour of the aqueous deposition of rocks, and against their consolidation by heat, rests on appearances not connected with this part of the subject; and though it be allowed, that the surface has been moved upwards from its original position, it by no means follows of necessity, that heat has been the source of the moving power. On the other hand, the elevation of the land is rendered probable, in a proportionate degree, by the difficulties that would attend its having been unmoved: while the existence of some mighty subter-

* Jameson, III, p. 201.

† D'Aubuisson sur les basaltes, &c. p. 93-4.

raneous

raneous force is established by several well-known phenomena. And that protrusion actually did take place, seems to be proved by the undoubted appearances of dislocation exhibited in veins, and perhaps not less distinctly by the great angular elevation of certain strata of rocks.

How far admitted.

On the question of elevation, however, this conclusion has been controverted. It is, indeed, admitted, that strata, composed "almost entirely" of mechanically divided matter, must have been deposited horizontally; but the inclined position in those of partially mechanical formation is ascribed, as we have seen, to the predominance of components chemically dissolved; and Mr. Jameson, though he enters into no detail of proof, has asserted as a "*fact*, that all inclined strata, with a very few exceptions, have been formed so originally, and do not owe their inclination to any subsequent change*."

Many strata, composed of matter, mechanically divided, are much inclined.

It would, nevertheless, be easy to point out strata evidently composed "almost entirely" of matter mechanically divided, which yet are very much inclined. It is, indeed, impossible to have examined rocks as they exist in nature, without having repeatedly observed them. The remarkable appearances at Valorrine, in the Alps of Switzerland, described by Saussure, have been selected by Mr. Playfair† as an instance of this description; and they are very well deserving of attention, both from the celebrity of the original observer, and the promptitude with which his inferences were deduced. The main fact exhibited at the place now mentioned, too, is this, that beds of a *coarse pudding-stone*, which, from their composition, must evidently have been formed in a position nearly horizontal, are actually found to be inclined to the horizon at an angle of almost 99°. The conclusion from these appearances occurred to Saussure at sight of them, upon the mountain itself; and the impression produced by what he saw, with respect to the probability

* Jameson, III, p. 55-56.

† Saussure. Voyages, 8vo. 3, § 688, &c. Playfair's Illustrations, § 40-41.

of

Inference of De Saussure, that great dislocations have taken place:

of vast changes of position in the strata of the globe in general, was such, that afterwards, on the summit of Mont-Blanc, where the strata are vertical, "placed as he now was," to use the words of Mr. Playfair, "on one of the highest summits of the earth's surface, he formed the bold conception, that the summit on which he was standing had been once buried under the surface, to the depth at least of one half the diameter of the mountain*." The strata of Mont-Blanc, however, are entirely crystalline, and therefore, on Werner's principles may have been originally much inclined; but a philosopher so cautious as Saussure would hardly have ventured to speculate so boldly as he did, in the present instance, if the facts on which he rested were not correctly ascertained; and if the observations be accurate, the strata at Valorrine must have been acted on as he supposes†."

Lapeyrouse, thinks they could not have been sudden:

It ought, however, to be noticed, on the other side of the question on the elevation of strata, that Lapeyrouse has stated, among his "conclusions," from observations on the structure of the Pyrenees, that the appearances of these mountains "do not allow the supposition, that any sudden or irregular movement had elevated strata originally horizontal‡."

What power did effect these?

But if this elevation be admitted, it will next be asked, what power is sufficient to have produced this mighty change? Here, however, it seems better to stop, and acknowledge, with Saussure§, that we can, at present, go no further than by attempting to account for these appearances, to pass into the regions of un-

* Illustrations, § 296.—Saussure, § 199.

† It has, indeed, been denied, on the authority of Friesleben, that the beds in question do really consist of pudding-stone, and the masses which Saussure considered as imbedded, are asserted to be of contemporaneous formation with the part in which they occur. But where there is a question of authority, we cannot hesitate to adhere to that of Saussure; nor are we confined to this particular instance for evidence upon the point at issue.

‡ Journal des Mines, No. 37, p. 65.

§ Voyages, § 690.

supported

supported speculation. The case is, in this instance, very different from that of the advocates of Werner, with respect to the disposal of their extraordinary primæval fluid. The fact of elevation is obvious from the most direct conclusion: whilst the existence and movements of their fluid, at a period almost beyond the reach of time, are, at best, but an hypothesis, opposed, as has been shewn, by the strongest physical objections, and, in truth, unnecessary for any purpose but that of supporting the system for which it has been gratuitously devised.

Huttonian speculation not admissible.

The convert to the Huttonian theory, indeed, will urge us to proceed in speculation, and will assert that, in his central fire, there is to be found power adequate to the effects we have described, the agency of which on fossil bodies is demonstrated, as he supposes, by evidence derived from the appearances of minerals themselves. But this reasoning cannot be admitted; for the existence of such a heat is not only contrary to the received laws of temperature; but the action of heat on mineral substances in general, seems to be disproved both by their individual appearances, and by several of their relations of position and structure.

Opinion of Werner respecting volcanoes;

This subject naturally leads to the consideration of the theory of *volcanoes*; for if there continue to act in nature any power adequate to the effects just mentioned, it must be sought for apparently in their explosive agency. And here again the opinion of Werner, that the activity of volcanoes is confined to the newest Flœtz-trap formation, appears to rest on grounds the most unstable. The effects and phenomena of eruptions, it is true, are so impressive on the imagination, that several observers who have witnessed their ravages, have assigned them an importance, in the present operations of nature, perhaps beyond the truth. But the depths from the surface to which their power seems to extend, the vast portions of the globe shaken, almost simultaneously, by *earthquakes*, and the mighty force that is necessary for their effects, which are evidently connected with some power resembling that of volcanic convulsion, are circumstances sufficient of themselves to prove the futility of Werner's conclusions, that "volcanic eruptions"

not probabl

are

are to be considered as "new occurrences in the history of nature;" and that "the volcanic state appears to be foreign to the earth*."

The South American volcanoes are of vast extent.

It is not in Europe, however, but in the Andes of South America, that the phenomena of volcanoes may be seen on the most instructive scale; and the effects which there occur are by no means of that trivial character that Werner's doctrine would lead us to imagine. The crater of Cotopaci is no less than 3000 feet in diameter, that of Rucupechensha about 4,800 feet; and the appearance of the surface in the adjoining countries corresponds with the magnitude of these enormous engines of destruction. The mountains of these regions being traversed in various places by crevices of vast depth, attributed by Humboldt to earthquakes; one, for example, at Chota, in Peru, is more than 5000 feet deep, while their breadth is, in many instances, so small, that they are compared by that traveller to *veins* which have not been filled up.

Volcanoes emit primitive compounds.

Sir James Hall has stated†, that amongst the erupted masses of Vesuvius there are some belonging to the class of primitive rocks; and the statement is confirmed by Werner, for *granites*, with other primitive compounds, are enumerated among the substances ejected by that mountain‡. A power, therefore, which, according to theory, is confined to the newest Flœtz-trap formation, the nearest to the surface of the globe§, must

thus

* Jameson III, p. 96.

† Paper on Heat under Compression, Edinburgh Transactions.

‡ Jameson, III, p. 213, 214; I. 56.

§ By this restriction we are cut off from the only channel through which an acquaintance with the interior of the globe could have been hoped for. For Werner rejects, as we have seen, the "bold conceptions" of Saussure, that the secrets of the greatest depths are to be looked for in the highest mountains; and the distance from the surface to which the works of art can penetrate is very insignificant. The deepest shaft at Krittenberg in Bohemia, goes down 500 fathoms (Jameson, III, 230,) which would bring us nearer to the centre in the proportion of 1 to about 7000, if it had been sunk at the level of the sea. Humboldt, however, states, that the deepest European mine is at 1338·6 feet in depth; and the great mine at Valenciana in Mexico

thus have torn from their place and erupted, rocks formed in the mass below them; and though it were denied, that these substances are primitive, the difficulty would not be diminished; for they are certainly quite unlike any of the *Flœtz-trap* family, and never could have had a place in that formation; and to suppose that they were torn from the sides of a subjacent mountain on which the volcano rests, is altogether an assumption as hard to be admitted as the difficulty it is intended to remove.

The observations of Werner himself on volcanoes were not extensive.

It becomes, indeed, a question whether Werner was justified in forming any decisive opinion upon this part of his subject. His own observations upon actually eruptive volcanos appear not to have been extensive; and he had never examined the regions where *extinct* ones were said to exist; his prejudices with respect to the latter being, in fact, so strong, that he continued, perhaps still continues, to deny their existence in Auvergne, a country which he never saw, in opposition to the testimony of the most intelligent observers of different schools, and even after some of the most distinguished of his own pupils had been compelled to renounce his opinion upon that instructive district; and as to volcanic *productions*, Mr. Jameson expressly says, that too little is yet known about them to enable him to describe them*.

Objections to Sir James Hall's theory of the extended agency of volcanic heat.

But if the importance of volcanic power, or some analogous agent, in the formation of the globe, has been too little attended to by Werner, Sir James Hall has assigned to it effects in the present economy of the mineral kingdom, apparently no less remote from probability; having gone so far as to infer, from his experiments and observations, the still continued and transferable agency of volcanic heat as a cause of the induration of rocks on successive portions of the interior of the earth; and thus with much ingenuity endeavoured to remove one of the strongest objections to the tenets of Dr. Hutton. But to heat, in any form, as the cause of consolidation, there

the bottom of which is still at a great height above the sea, is only 1693 feet deep.

* Jameson, III, p. 215.

are

are still the objections derived from the appearances and properties of minerals, which no hypothesis of this description can annul. Nor can volcanoes, whatever may have been their power, be admitted to possess now the diffusive mode of action thus supposed. If they were still, as Sir James Hall imagines, the unwearied artificers of revolution and regeneration, ought they not to be expected sometimes to reveal themselves, and to break forth in new and various places? Whereas the scenes of their activity are fixed; and their immediate ravages confined; nor have new ones, in any single instance, appeared in countries which did not bear evident marks of previous volcanic agency within the reach of historical record.

Werner's arrangement of rocks.

The *arrangement of rocks*, which Werner has adopted, is founded on the order of their superposition, in combination with his theory. He forms of them two great divisions, *Aquatic* and *Ignigenous*, and five classes denominated *Primitive Transition, Flœtz, Alluvial,* and *Volcanic*; and these are subdivided into forty-six sections, of which some, in the Flœtz-class, are "compound formations," and consequently include rocks of several different kinds.

His classes do not correspond with his supposed stages of deposition.

But though connected with his theory, the classes of Werner do not correspond with the stages of deposition, which he supposes to have taken place. The first or primitive class appears to do so; it is marked by an era in the supposed history of the globe:—but from thence to the second and most important return of the dissolving fluid, the theory mentions but one uninterrupted retreat; the rocks, therefore, which such a movement should produce, ought all to have "conformable" arrangement; and, in point of fact, all those of the second and third classes, except the newest Flœtz-trap formation, do form in characters and composition a continued series; "Greywacke is a complete sandstone*;" from thence the admixture of matter mechanically divided, goes on increasing; and petrifactions, also, progressive in quantity, and in the scale of organization, pervade the whole. There remains, then, as sources

* Jameson, III, p. 93.

of

of distinction between the transition and Flœtz-rocks, only their general relations of position and structure; and by neither does it appear, that any well-marked boundary can be traced between them; at least if such distinctions do exist, they have not yet been pointed out explicitly; whilst, on the other hand, the newest Flœtz-trap rocks are both in their characters and supposed mode of formation, perfectly distinguished, and ought, on Werner's own principles, to have been detached from the preceding part of the arrangement.

Werner's descriptive terms are objectionable, because they mostly express opinions and not facts:

To the names of the classes and the descriptive terms introduced by Werner, it may justly be objected, that many of them are founded upon theory, expressing, consequently, not facts, but opinions; not the appearances of nature, but the suppositions by which he endeavours to explain them. Of the five classes the denominations of three are of this description—*primitive*, *transition*, and *alluvial*; and the terms *new* and *old*, with those of *first* and *second*, so far as they refer to time, which contribute to the denominations of several important divisions of rocks, are constantly employed in describing them, come likewise under this objection, though they may appear, at first view, merely expressions of fact. The term *formation*, also, is evidently theoretic, alluding, not very definitely, to the supposed progress of nature in the deposition of rocks, and strictly founded on assumption; for it is evident, that the successive substances in any of the compound, or even of the simple formations, though now in contact, *may* have been deposited at distant periods of time. In describing natural bodies, all that can, with safety, be referred to, are the appearances or properties which they present; and these, in the present instance, ought to have been conveyed by terms such as group or series, expressing only the fact of justa-position.

Doubt whether a language founded on theory can be in any case beneficial.

Even where theory is much more free from error than the most sanguine admirers of Werner can assert his speculations now to be, the propriety of connecting with it a system of language is very doubtful. The rapid progress of modern chemistry has been supposed to prove the advantages of names derived

derived from theory. But such terms are injurious in proportion as the speculations with which they are connected are erroneous; and even in chemistry it will, perhaps, appear hereafter, that they have rather assisted in diffusing a brilliant system, than advanced the progress of truth. But in geology to form a nomenclature upon similar principles, is evidently premature; and, in the mean time, such terms circumscribe the space through which useful knowledge can be diffused. Observations stated in language expressing only facts, are universally intelligible; while the writings of Werner's pupils, stamped with his insignia, are restricted to circulation in their particular school.

Denominations of rocks from their actual position may, perhaps, be the most eligible.

Perhaps, then, after all, the denominations *primary**, *secondary*, *tertiary*, and *volcanic*, might still be more eligible for the classes of rocks. The first term to comprehend the present primitive class of Werner, the second his transition, Flœtz, and newest Flœtz-trap rocks; (in which sense the term *secondary* is used by D'Aubuisson, and other French mineralogists of the Wernerian school.) In each of these classes there might be a corresponding division into *conformable* and *overlying* formations; and the present Flœtz might, if necessary, be further distinguished from the transition rocks. The *tertiary* class would comprehend the alluvial rocks of Werner, and the *volcanic* those to which he has given that denomination. And further distinctions or subdivisions might be well expressed by terms denoting merely order of succession, or well-marked circumstances of character and composition. By such denominations nothing hypothetic would be implied; and if, as is asserted, the theory of Werner be truly grounded on the phenomena, the arrangement would thus be rendered more conformable to it, though freed from the objections involved by terms implying such a connection.

* Without a familiar knowledge of German, it is hazardous to observe on terms borrowed from that language. But *Urgebirge*, the original name for this class, appears to signify, not primitive, but very old, of revered antiquity; in which sense it comes near to *primary*.

With

With respect to composition, rocks are divided by Werner to *simple* and *aggregated*; the former, consisting of unmixed mple minerals, are named and described according to his *yctognostic* system; and to express the structure of those the latter division, he has framed a new descriptive language, hich has been very highly praised by his pupils, and to which, transferred from the original German by Mr. Jameson, perps the strongest objection is the harshness of its terms. The ecies of *aggregated rocks* are about eighteen in number; the *nple*, including those detailed in the formations of the newest œtz-trap, are about thirty-five; and if the various componts of the aggregates, rejecting the less important, be included the enumeration, the total number of mineral species contuting the great mass of the globe, will not amount to more an twenty-five or thirty.

Description of rocks as to their composition.

The selection of the compounds which Werner has chosen his species of rocks, is founded entirely upon their *geognostic* portance; that is to say, upon the distinctness and extent the masses which they present in nature, when viewed on great scale. Several combinations of the simple minerals ing thus judiciously considered merely as varieties of the ore important ones. The propriety of the selection is prortioned to the extent and fidelity of the observations of this uralist; but several of his species must still be regarded as ovisional, which future inquiries will modify and improve.

Species of Werner founded upon their geognostic importance.

The *nomenclature* of rocks adopted by Werner, has not been y happily chosen. In a branch of knowledge founded, like mineralogy of the German school, on materials brought ether by unenlightened labourers, much barbarism of names s naturally to be expected. A reformer must have been itent to compromise between the principles of correct nonclature, and the authority of terms already sanctioned by tom. Accordingly, in those formed and selected by Werner, unity of system is not to be looked for; some of the names, haps the best of them, being virtually insignificant, while ers are grounded on characters extremely variable and unportant. Of the latter description those derived from colour are

Nomenclature not happily chosen; terms authorized by custom being necessarily admitted.

are probably the most unhappy; Mr. Jameson having found it necessary to defend himself for having, in strict conformity with his master, called a compound *green stone*, which is actually red*; (the more unfortunately as the red colour of the felspar is stated to be one of the distinctive characters of *sienite* from green stone† ;) and it would be easy to point out similar instances of incongruity.

Arrangement and language.

The limits to which an essay of this nature is confined, prevent the pursuing into further detail that part of the subject which relates to arrangement and language, the present object having been, chiefly, to state the leading features of the structure of the globe according to Werner, and the principles which have guided the fabrication of his theory. A comparison of his nomenclature and arrangement with the rules given by Linnæus in the department of the Philosophia Botanica, which apply, with equal propriety, to every branch of natural history, will readily suggest further observations.

The Wernerian system exhibits a most valuable mass of facts, with little of philosophical connection or explanation.

To conclude,—the Wernerian system of geognosy is, perhaps, truly to be regarded as an elaborate and valuable accumulation of facts, brought together without much philosophical connection, and attempted to be explained by hypotheses very feebly supported. It has been useful, and may still contribute to the progress of the subject, by binding together what otherwise would have been dispersed; but it must speedily give place to something more correct in its general principles, and more minutely accurate in its detail. These remarks, however, are by no means intended to detract from the merit of Werner's researches, nor to deny to him the credit of having produced an era in the science of the earth. The developement of the great formations of rocks, and of the constancy of their order of succession throughout the globe, if the latter be confirmed by future observations, would be sufficient alone to place him very high amongst the cultivators of

It places him very high among the cultivators of science;

* Mineralogical Survey of Dumfries, p. 51. 168.

† Geognosy, p. 40.

of

of natural knowledge. These results may be studied without connecting them with any theory; and the light which they have thrown upon the structure of the globe will continue to have good effect, whatever may be thought of the speculative powers of the discoverer.

Let those who are dissatisfied with this restricted praise remember, that in chemistry the system of Stahl, once thought impregnable, is now almost forgotten; that even the more refined doctrines of Lavoisier are hastening to their fall: and that, if the credit of a perfect geological theory be denied to Werner, the period seems not yet to have arrived when *any* theory of the earth can be successful. Negative conclusions, indeed, may now be deduced, and detached propositions may be established; but before they are connected as parts of a consistent whole, our knowledge of the structure of the earth must be increased by new and large accessions of correct description; our acquaintance with the laws of chemistry, and of electric and magnetic polarity, must be much extended; and our opinions respecting the latter obscure but powerful agents, must probably undergo a considerable change.

but like the systems of Stahl and Lavoisier must give place to approaching discoveries.

---

## II.

## *On the Measure of Moving Force. By Mr.* PETER EWART.

*(Continued from p. 97.)*

I am quite at a loss to understand why Mr. Atwood excluded this case from those in which the moving force may be estimated by the products of the matter into the squares of their velocities. If, in cases of rotatory motion about fixed axes, that principle "obtains," as he observes, "without exception," there can, I think, be no exception to its application in cases of this description.

Cases of difficulty in the doctrines of moving force.

Having gone through the examples of force producing motion from a state of rest, we come now to the examination of

cases

Cases of difficulty in the doctrines of moving force.

cases where motion is destroyed, or where it is transferred from one body to another.

It was a favourite doctrine with the Cartesians, and it was maintained also, though upon quite different principles, by Leibnitz and John Bernoulli, that motion could not be lost; for the same quantity of motion or of force, is was said, must be always preserved in the world. A similar doctrine, applied to explain the collision of soft bodies, has been supported by authors of later date; and if it were admitted that we have no indication of the loss of force, unless motion be lost in the centre of gravity of the system in which the force acts, it might truly be said that no force can be lost.

It has never been questioned that motion may be generated, accelerated, or retarded in a variety of ways, and there appears to be no good reason for supposing that it may not be destroyed as well as generated.

It was Sir Isaac Newton's opinion, that motion may be lost, and he has given many familiar examples of the manner in which it is lost. "It may be tried," he says, by letting two equal pendulums form against one another from equal heights. If the pendulums be of lead, or soft clay, they will loose all, or almost all, their motion." In the same way the motion of A and B (case 6th) is lost when the spring is compressed. This case has been so often brought forward, and so much has been said about it, on both sides of the question, that it may appear strange that I should produce it again. I shall endeavour to confine my observations upon it in a small compass.

It is very generally understood, and it has been received almost as an axiom, that if two bodies meet and destroy each other's motion, their quantities of motion, and their respective forces, must therefore be equal. Dr. Reid has given a better enunciation of this proposition. He says, "If two bodies meet directly with a shock, which mutually destroys their motion, *without producing any other sensible effect*, it may be fairly concluded, that they meet with equal force*." Now this is a fair

* Essay on Quantity—Phil. Trans. 1748, p. 515.

reference

reference to experiment, and, in the case under consideration, we certainly have a measurable, "sensible effect" in the compression of the spring, which cannot be produced without force. But although the ends of the spring meet at E, (fig. 6) it is still held by many that that effect is produced equally by A and B. If the forces of A and B are really equal, we should have the same effect produced when we substitute for B another ball equal in weight and velocity to A. But the same effect cannot be produced by that means, and if the real effects be examined, we shall always find that the spring is less compressed (as measured by the pressure into the space) by A than by B in the ratio of 1 to 2.

Cases of difficulty in the doctrines of moving force.

It is true the common centre of gravity of A and B remains undisturbed; but is it necessary that we should confine our attention solely to that centre of gravity? If we find that the motion of a body cannot be destroyed without producing certain measurable effects of force, and if we find these effects to bear an unvarying relation in quantity to the motion destroyed, there surely can be no inconsistency in taking the amount of these effects for the measure of the force of the moving body.

I confess I have never been able to understand M. D'Alembert's distinction between the *sum* and the *number* of the obstacles overcome*. If the obstacles be equal to each other, it can make no difference whether their sum or their number be taken as the measure of the force. If they be unequal, the sum of their separate amounts must surely be the absolute quantity of resistance overcome, and the proper measure of the force by which it is overcome. To say that the quantities of resistance, during infinitely small instants of time, must be equal to each other, is assuming a most unreasonable postulatum. The difficulty cannot be removed by taking insensible, instead of sensible, portions of time: for we have no reason to suppose, that the pressure into the space approaches nearer to equality in infinitely small, than in palpably large, portions of time.

* See page 151.

 This

Cases of difficulty in the doctrines of moving force.

This compression of the spring is comprehended by Mr. Smeaton under the term *change of figure*; and he has shown, by some well-chosen experiments, that when a non-elastic yielding body, moving with a given velocity, strikes directly another equal body at rest, exactly half the force of the striking body is expended in producing change of figure*.

The facts exhibited in the 7th case are similar to those which Mr. Smeaton has described as the results of his experiments. According to the theory, the whole force of A (fig. 7) before collision, is to be found in the *motion* of A and B after collision. But if that be admitted, we must suppose the spring to have been compressed without force: yet we have no more reason to suppose that the spring can be compressed without force, than that a body can be put in motion without force; and the amount of the force which has been expended in compressing the spring, is ascertained by its effects in producing motion in C and D; and although these balls move in opposite directions, it cannot be supposed that their motion can be produced without force.

In this explanation, however, of the action of the spring on C and D, Mr. Maclaurin understood a material inconsistency to be involved, which he stated in a treatise that obtained the prize of the Royal Academy of Sciences at Paris, in 1724†. Mr. Maclaurin supposes two equal bodies like C and D, with the compressed spring between them, to be situated in a space E F G H, which, together with the balls, "moves uniformly in the direction CD with the velocity as 1; and that the spring impresses on the equal bodies, C and D, equal velocities in opposite directions, that are each as 1. Then the absolute velocity of D (which was as 1) will be now as 2; and according to the new doctrine, its force as 4; whereas the absolute velocity, and the force of C (which was as 1) will be now de-

* Experiments on Collision—Phil. Trans. 1782.

† The "Discours sur le mouvement" of John Bernoulli was offered for the same prize, but was rejected, the preference being given to the treatises of Maclaurin and Maziere.

stroyed;

stroyed; so that the action of the spring adds to D a force as 3, and subducts from the equal body C a force as 1 only; and yet it seems manifest, that the actions of the springs, on these equal bodies, ought to be equal; (and M. Bernoulli expressly owns them to be so) that is, equal actions of the same springs upon equal bodies would produce very unequal effects, the one being triple of the other according to the new doctrine, than which hardly any thing more absurd can be advanced in philosophy or mechanics*."

Cases of difficulty in the doctrines of moving force.

This argument of Mr. Maclaurin has always been considered as the most ingenious and the strongest objection that has been brought against the principle of the *vis viva*. But we have the following remarks upon it from Dr. Milner: "I shall only just observe, that if M. Bernoulli expressly owns, that springs, interposed between two bodies in a space, which is carried uniformly in the direction in which the springs act, will always generate equal forces in the bodies according to his own definition of the term, he talks more inconsistently than I have observed him to do. On the contrary, if I could find that he has answered this famous argument (which Dr. Jurin proposed over again in the Philosophical Transactions, vol. xliii. with a conditional promise of embracing the Leibnitzian doctrine) by simply saying, that springs he considers as moving forces, or, when the bodies are equal, as accelerating forces; and that their actions are equal, when in equal times they generate equal velocities, but not necessarily equal forces in the equal bodies; I should not make the least scruple to own, that I thought his reasoning solid and conclusive, and his distinctions a full answer to every objection of that sort." To this, Dr. Milner has added the following note: "No doubt Mr. Maclaurin refers to the following passage of Bernoulli: La force du choc, ou de l'action des corps les uns sur les autres, depend uniquement de leurs vîtesses respectives; or il est visibles que les vîtesses respectives des corps ne changent pas avant le choc, soit que le plan ou l'espace qui les contient soit sans mouve-

* Account of Sir Isaac Newton's Discoveries, book 2, chapter 2.



ment,

Cases of difficulty in the doctrines of moving force.

ment, soit qu'il se mouve uniformement, suivant, une direction donnée, les vitesse respectives seront donc encore les mêmes apres le choc. This quotation puts the matter beyond dispute. It is plain, Bernoulli, though he makes use of the word action, is only speaking of the motion lost or communicated, and the relative velocities of the bodies; there is not the most distant hint at the change in their absolute forces."

In addition to this, I would, with great deference, observe, that by the term *equal actions of the spring,* as used above by Mr. Maclaurin, *equal pressures* only are meant: but M. Bernoulli held that the motion of a body cannot be produced by mere pressure. Unless the pressure act through some portion of space, no motion can be produced; and if, together with the pressure, we take into consideration the space through which the pressure acts, we shall find that, while the motion of C has been transferred to D, the whole force of the spring has also been communicated to D. This will become more obvious in examining the 8th case. It should be observed, too, that when Mr. Maclaurin sets out with supposing the bodies to be in motion, and the spring to be in a compressed state, he refers to a previous application of force, of which he takes no farther notice, although a part of this previous force is afterwards expended or given out, in producing the changes which he describes.

It is true our researches must be limited chiefly to relative motion---Of absolute motion we know but little. But is not the motion of the space EFGH with the velocity as 1, relative motion with regard to some supposed point, as much as the final motion of D is relative motion?

If the compressed spring be disengaged while the bodies are at rest, the motion of the bodies is acknowledged to be produced by the force of the spring: but when the space EFGH and the bodies are supposed to be put in motion before the spring is disengaged, there is, according to the prevailing theory, no motion produced by the spring. There is merely a transfer of motion from C to D, and we have only the same motion after, that we had before, the action of the spring.

Is

Is there not some inconsistency in supposing the spring to produce motion in one case, but none in the other?

Cases of difficulty in the doctrines of moving force.

If, instead of the unequal pressure of a spring, an uniform pressure be applied, as in the 8th case, the various quantities of mechanical force expended at different periods of the operation, will be more distinctly shown; for, the pressure being constant, each portion of space through which it acts, will express the quantity of mechanical power which has been expended in that space.

In its passage through a space $=$EH$=\frac{3}{4}$ EF (fig. 8) an uniform resistance has been opposed to A, which would bring it to rest in a space $=$ EF. When it has arrived opposite to H, it has therefore lost half its velocity; and B having arrived opposite to I by the action of an equal pressure through a space $=$ FI $=\frac{1}{4}$ EF has acquired the velocity $\frac{v}{2}$; and KH, equal $\frac{1}{2}$ EF, will consequently be the depth of the penetration of $c$ into A. Now, if A be a nonelastic soft mass of clay, for example, we know that it cannot be penetrated without force; nor have we any reason to suppose, that the force which has been expended in producing the penetration, can ever be restored. We therefore cannot expect to find in the motion of A and B, after collision, the same quantity of force which they had before collision. If, however, the pressure into the space through which it acts be taken as the measure of the force, we shall find that a compound effect has been produced by A in its passage through the space $=$ EH, that only one-third of the force which A has lost has been communicated to B, and that the other two-thirds of that force has been spent in producing a change of figure in A. These proportions are obvious from the mere inspection of the diagram. We may suppose A to be a much harder substance than clay, so that the space represented by EF may be very small; but the pressure being proportionally greater, the product of the pressure into the space will still be the same, however small the penetration may be.

Any explanation, however, which takes into consideration the force which is expended in producing a change of figure, is

Cases of difficulty in the doctrines of moving force.

is strongly objected to by all those who hold that the product of the mass into its velocity is the proper measure of the force of a body in motion. They contend that "all the experiments which are usually brought to determine the impressions made upon soft bodies, as snow, clay, &c. are absolutely unfit for the purpose." That "the circumstances which take place in the production of these effects, are such as we can never discover." And that "the directions in which the particles recede, the velocities they acquire, their mutual actions upon one another, and lastly, the time in which these effects are performed, are all beyond the reach of computation*."

To this it may be replied, that if only the pressure and the space through which it has acted be determined, it would be quite superfluous to enter into any farther computation of the circumstances above enumerated, in order to estimate the quantity of mechanical force expended in producing the impression. For, whatever may have been the relative directions, velocities, or mutual actions of the particles during the time that the impression was making, no internal motion remains after the impression is completed; and the force can have been spent in no other way than in compressing the particles together, or in overcoming their tenacity. To take a familiar example.---If a quantity of corn is to be ground, a considerable quantity of motion must, no doubt, be produced before that can be effected; but after it is ground, there is no more motion in the flour than there was in the corn before it was ground, and the whole force employed must have been expended in overcoming the tenacity or cohesion of the particles of the corn.

In answer to the very common objection, that the quantity of force expended in producing an effect of this kind, cannot be precisely ascertained, it may be observed, that in real practice, such quantities of force are estimated with quite as much precision as the force necessary to generate a given velocity in a given mass, in projecting a cannon ball, for example. The application and measurement of mechanical force producing

* Dr. Milner. Phil. Trans. 1778, p. 353.

changes

changes of figure, are, indeed, the chief occupations of practical men in the construction and management of machinery.

Cases of difficulty in the doctrines of moving force.

The force spent in producing change of figure in the collision of bodies, was noticed by John Bernoulli in his dissertation De vera notione virium vivarum, as follows. "Si corpora non sunt perfecte elastica, aliqua pars virium vivarum, quæ periisse videtur, consumitur in compressione corporum, quando perfecte se non restituunt; a quo autem nunc abstrahimus, concipientes, compressionem illam esse similem compressioni elastri, quod post tensionem factam impediretur ab aliquo retinaculo, quo minus se rursus dilatare posset, et sic non redderet, sed in se retineret vim vivam, quam a corpore incurrente accepisset: unde nihil virium periret, etsi periisse videretur*."

From this passage, and from various other passages in his works, relating to the doctrine "de conservatione virium vivarum," it appears, that Bernoulli thought it necessary to maintain that no force could be lost, and that even in the collision of nonelastic bodies he considered the change of figure to be such, that the force which had been expended in producing it might be recovered by the restoration of the figure, or by some other means. Why he considered it incumbent upon him to maintain such opinions, or upon what foundation he understood them to rest, it is hard to say. Experience furnishes us with nothing which can justify the conclusion, that the force spent in producing change of figure in nonelastic bodies, can ever be restored.

I believe Mr. Smeaton was the first who subjected to actual admeasurement the force spent in producing change of figure in the collision of nonelastic bodies†. He appears to have been led to this investigation, not by curiosity merely, but by a conviction of the insufficiency of the prevailing doctrines of forces to account for the facts which were constantly presented to him in his ordinary occupations, and particularly, as I have before observed, in the action of water on water wheels. It is

* Bernoulli's works, vol. iii, p 243.

† Phil. Trans. 1782.

very

Cases of difficulty in the doctrines of moving force.

very remarkable, that while Mr. Smeaton's other dissertations on the principles of moving force, have met with considerable attention abroad as well as at home, this last treatise on the collision of bodies, (which he himself considered a most important one, as containing the best confirmation of his former conclusions) has been almost totally neglected by all succeeding writers. It is impossible for me to do justice to it by giving an abstract of it; but I would earnestly recommend the entire treatise to the attention of all those who take an interest in investigations of this kind.

With regard to the collision of bodies, which are supposed to be perfectly hard, as well as nonelastic, Mr. Smeaton understood a contradiction to be involved in the very supposition of the existence of such bodies. It has never been contended, that any such are to be found in nature. But it is very generally argued, with Mr. Maclaurin, that "there is the same objection (of non-existence) against admitting and treating of bodies of a perfect elasticity*." In reply to this I would observe, that the objection does not appear to be of the same weight against perfectly elastic, as against perfectly nonelastic hard bodies. For we have substances which approach very nearly to perfect elasticity; but we can find no substance of which the qualities approach to hardness and nonelasticity united. In general, the elasticity increases as the hardness increases, and no substance has ever been produced that can be called hard, without possessing, at the same time, great elasticity.

It does not appear that the possible existence of a perfectly hard nonelastic body was obvious to the first discoverers of the laws of percussion. Huygens appears to have understood a hard body to be one that is perfectly elastic. His 6th law of percussion is as follows: "Summa productorum factorum a mole cujuslibet corporis *duri* ducta in quadratum suæ celeritatis, eadem semper est ante et post occursum eorum†."

M. Laplace considers that "Ce principe" de la conservation

* Account of Sir Isaac Newton's Discoveries, p. 98.

† Phil. Trans. 1669, p. 928.

des

des *forces vives* " n'a lieu que dans les cas, où les mouvemens des corps changent par des nuances insensibles. Si les mouvemens éprouvent des changemens brusques, la force vive est diminuée d'une quantite que l'on determinera de cette maniére ;"——* and taking it for granted, in the usual way, that where the change of motion is *sudden*, the bodies must be nonelastic, he investigates the motions which are known to result from the collision of nonelastic soft bodies. But that conclusion is not justified by experience ; for the characters of elasticity are often the most apparent where the changes of motion are, at far as we can judge, the most sudden.

Cases of difficulty in the doctrines of moving force.

The supposition of the possible existence of a perfectly hard body, appeats to involve another inconsistency which I will endeavour to state in a few words. The resistance, or pressure, against *c* (fig. 8) being increased, and the depth of its penetration being diminished, in proportion as the hardness of A is increased, it follows, that if, by supposing A to be perfectly hard, the depth of the penetration be reduced to nothing, the pressure must be increased to infinity. That is, the pressure must be infinitely great to communicate even the smallest finite quantity of motion. But I believe the " law of continuity" is not so much objected to now as it was formerly, and few will be disposed to contend, that a body may, from a state of rest, arrive at any given velocity, without passing through the intermediate degrees of velocity between that and rest ; and consequently few will now contend for the possible existence of a perfectly hard substance.

If, instead of a nonelastic soft substance, we suppose A to be a hollow sphere filled with a dense elastic fluid, and *c* to pass through a hole in the side of the sphere, so as to move without friction and be uniformly pressed outwards by the fluid, A will then represent a perfectly elactic body.

It may be proper to observe, that although we suppose *c* to make no penetration into B, we do not suppose B to be perfectly hard. We only suppose it to be so much harder than A, that the penetration shall be very small when compared with the pene-

† Méchanique céleste, vol I. p. 52.

Cases of difficulty in the doctrines of moving force.

penetration into A. If we were to suppose A and B to yield equally to $c$, the same explanation of the phenomena as when A only is supposed to be penetrated, will strictly apply; only the diagram would be a little more complicated

Let us now suppose the first part of the operation in the collision of A against B, to be the same as already described in the case of a soft body, and supposing them to be in the situation as represented at No. 2, let us observe what must follow. When A has arrived opposite to F, as represented at No. 3, $c$ will have returned to its original place with respect to A, and B will have arrived opposite to G (FG being = EF), A will be at rest, and B will have acquired the full velocity $v$. Now, it is obvious, that if A had not moved on from its position No. 2, $c$ would, in this last part of the operation, have acted upon B only till it arrived opposite to L (FL being $= \frac{3}{4}$ EF), and its final velocity would have been only $\sqrt{\frac{3}{4} v^2}$. But A having moved on to its place No, 3, $c$ will have acted on B till it has arrived opposite to G; and the force which has been lost by A in its passage through the space = HF, as well as the force of $c$ through a space = HK, has been communicated to B. In other words, the force which, in the first part of the operation, had been expended in producing the change of figure, has, in the last part of the operation, been reproduced by the expansion of the figure to its original state, and has, together with the remaining force of A, been communicated to B. If this explanation be applied to the change of motion produced in C and D fig. 7, as referred to at page 185, it must be obvious, I think, that when C is brought to rest, the force which it has lost, and the force of the spring, have both been communicated to D.

In the collision of unequal masses, the distribution of the force is rather more complicated. Let M (fig, 15) be immoveable, and filled with a dense elastic fluid, so that N, moving with the velocity $v$, and meeting with an uniform resistance, would be brought to rest by driving the cylinder C up to O. Then, if we suppose M, = 2N, to be in free space, and if we divide OP, = OR, into nine equal parts, and make

OS

$OS = \frac{1}{9} OR$, it will be obvious, that when N has arrived at $S'$, its velocity will be $\frac{v}{3}$, and M will, at the same time, have arrived at $2'$, and will have acquired the velocity $\frac{v}{3}$, and the penetration of C into M will be $\frac{2}{3}$ OR. In this part of the operation, then, N has, (on the principles adopted in explaining the last case) lost, or rather given out, $\frac{8}{9}$ of its force; of the effects of which $\frac{2}{9}$ are found in the acquired motion of M, and $\frac{6}{9}$ in the change of figure of M. In the next stage of the operation N will have arrived at $O'$ and be at rest: M will have arrived at 4·5, and will have acquired the velocity $\frac{v}{3}$. And lastly, when M has arrived at $8'$, it will have acquired the velocity $\frac{2}{3}v$, and N will have moved back to $S''$, and will have re-acquired the velocity $\frac{v}{3}$, and the balls will be at the same distance they were at first when N struck C. Inexplaining these facts by the common theory, it is admitted, that N has communicated to M a greater quantity of motion than it had; that inconsistency, however, is supposed to be removed by saying, that the motion of N being in the contrary direction, it must be deducted from the motion of M, and the remainder will be equal to the original motion of N. But we know that a body cannot be put in motion, in any direction, without force, and as the final motion of N, as well as that of M, must have been derived from the original force of N; it appears that the motion of N should be added to, instead of being deducted from, the motion of M, before we can properly compare the effects with the force by which they have been produced. If N had remained at rest at $O'$, M would have been acted upon by C till it arrived at $9'$, and the whole original force of N would have been found in the motion of M, which would finally have acquired the velocity $\sqrt{\frac{v^2}{2}}$.

Cases of difficulty in the doctrines of moving force.

This last explanation is given by Dr. Wollaston as follows. "But there is one view," he observes, "in which the comparative

Cases of difficulty in the doctrines of moving force.

parative forces of impact of different bodies was not examined by Smeaton, and it may be worth while to show, that when the whole energy of a body A is employed without loss in giving velocity to a second body B, the *impetus* which B receives is, in all cases, equal to that of A, and the force transferred to B, or by it to a third body C, (if also communicated without loss, and duly estimated as a mechanic force,) is always equal to that from which it originated.

"As the simplest case of entire transfer, the body A may be supposed to act upon B in a direct line through the medium of a light spring, so contrived, that the spring is prevented by a ratchet from returning in the direction towards A, but expands again entirely in the direction towards B, and by that means exerts the whole force which had been wound up by the action of A, in giving motion to B alone*."

In the explanations which I have offered of the phenomena which occur in the collision of bodies, I have supposed all the changes of motion and of figure to be gradual, not instantaneous; and it may be objected to these explanations, that they cannot be applied to cases of instantaneous impact. But I believe it is now generally admitted, as I have already observed, that impact cannot be perfectly instantaneous, that some small but finite portion of time must pass during the operation†; and if this be so, the changes of motion must occupy also some portion of space. Now, if we suppose that portion of space to be magnified by means of lenses, we cannot doubt that we should see all the changes of figure, as well as of motion, distinctly in their order, the same as they actually appear when they are gradually produced in extended spaces, and the same explanations may be strictly applied to the changes which take place in the smallest as well as in the largest spaces.

The 9th case is stated merely to show, that we cannot form a just estimate of the forces of bodies in motion, by attending solely to the *quantity of motion* of their common centre of

* Phil. Trans. 1806, p. 19.

† See Hutton's Dictionary, art. Force, vol. I, p. 496.

gravity;

gravity; and that, in cases of composition of motion, wherever there is a loss of mechanical force *in any direction*, there must be a corresponding *change of figure*, which may always be estimated upon the principles adopted in the preceding cases.

Cases of difficulty in the doctrines of moving force.

In the 10th case, the *quantity of motion* of A (fig. 10) after collision, is the same as that of the common centre of gravity of E and F before collision. But the whole forces of E and F are not exhibited in the *quantity of motion* of their common centre of gravity. The motion of A, however, is the whole effect produced, and if we estimate its force by its mass into its velocity, we cannot account for the total loss of the forces of E and F; but if we estimate all the forces by the masses into the squares of their separate velocities, the agreement between the forces and their joint effect is obvious.

I have already adverted (page 134) to a statement of a case of composition of motion made by M. Laplace, in which a hypothetical relation of the force of a body in motion to the square of its velocity is adopted, and where the supposed effects would be quite at variance with those of experience. It will, perhaps, be better understood with a reference to this 10th case.

M. Laplace says, "La force peut être exprimée par une infinité de fonctions de la vîtesse, qui n' impliquent pas contradiction. Il n'y en a point, par exemple, a la supposer proportionnelle au carré de la vitesse. Dans cette hypothèse, il est facile de déterminer le mouvement d'un point solicité par un nombre quelconque de forces, dont les vîtesses sont connues; car si l'on prend sur les directions de ces forces, à partir de leur point de concurs, de droites pour représenter leurs vîtesses, et si l'on détermine sur ce mêmes directions, en partant du même point, de nouvelles droites qui soient entre elles, commes les carrés des premières; ces droites pourront représenter les forces elle-mêmes. En les composant ensuite par ce qui précède, on aura la direction de la résultante, ainsi que la droite qui l'exprime, et qui sera au carré de la vîtesse correspondante, comme la droite qui représente une des forces composantes, est au carré de sa vîtesse. On voit par là, comment on peut déterminer le

mouvement

Cases of difficulty in the doctrines of moving force.

mouvement, d'un point, quelle que soit la fonction de la vitesse que exprime la force*."

Now, if AB (fig. 10th) be produced to G, and AC to H, making $AH : AC^2 : : AG : AB^2$, and if we complete the rectangle, and draw the diagonal AI, we shall have a diagram of the construction described above by M. Laplace; and, if I understand him right, he concludes, that if the forces of E and F are respectively as the squares of their velocities, AI must be the resulting direction of A, and the square of its velocity must be to AI as $AB^2 \cdot AG$. If, by the force of a body in motion, being as the square of its velocity, it were meant, that the pressure exerted in bringing it to rest in a *given time*, must be as the square of its velocity, the result must, no doubt, be such as M. Laplace describes. I cannot find, however, that this meaning has ever been applied to the principle in question. Such a hypothesis could not be entertained, indeed, for a moment, without setting aside the incontrovertible explanations and conclusions of Galileo. In answer to the objection implied, in the reasoning of M. Laplace, against the force being as the square of the velocity, I can only repeat what I have already so often repeated, that it is not the pressure exerted in a *given time*, but the pressure exerted through a *given space*, that is understood to be universally as the mass into the square of its velocity; and I may add that there is nothing hypothetical in this conclusion. Being derived from an induction of facts, it must stand or fall with the facts on which it is grounded.

In the next case, where the angle BAC (fig. 11) is not a right angle, the results after collision are, in two respects, different from the last. E and F are not at rest after collision, and the *quantity of motion* of A is not the same as that of the common centre of gravity of E and F before collision†. This

* Système du Monde, p. 141.

† In describing this case at page 123, I have omitted to state, that E and F are supposed to move with equal velocities; but it will be obvious, from the figure, and from the results which are given, that it was so understood.

case,

ase, or rather the converse of it in a less simple form, was first explained by John Bernoulli, in the eleventh chapter of his " Discours sur le Mouvement," and the solution which I have given (page 123) will be found to agree with his. In his twelfth chapter, however, he extends his solution to the case where a ball D (fig. 16) strikes any number of pairs of balls, the balls in each pair being equal and at equal distances from the line of direction of the striking ball. But that solution, as it has been justly observed by Mr. Robins, will be true only when the same time is taken up in communicating motion to all the balls*," and that cannot take place unless a peculiar modification of the elasticity be adapted to the respective masses and positions of each pair of balls at their points of contact; and even then the results will not always be as they are laid down by M. Bernoulli. His solution, therefore, was not, what he understood it to be, a general one.

Cases of difficulty in the doctrines of moving force.

Cases of this description appear to have been imperfectly understood at the time when M. Bernoulli wrote. In the " Histoire de l'Academie Royale" of Paris, for the year 1721, p. 84, the following case is stated. Two equal balls moving with equal velocities, are supposed, as in the eleventh case, to strike, at the same instant, a third ball at rest; and the directions AC and AB of the striking balls E and F, are supposed to be such, that we shall have AC or AB = 2 AH. That is, that the absolute velocity of E or F, before they strike A, shall be equal to twice the velocity of their common centre of gravity. And it is concluded, that AD will represent the velocity of A after the stroke.

It appears, also, that some of the most obvious effects of elasticity in the collision of bodies were as much misapprehended then as the motion of the bodies after collision. In the same department of the valuable work last quoted, for the year 1728, the same subject (sur la force des corps en mouvement) is resumed, and at page 77 there is the following statement.

" Un corps, qui a une vîtesse à parcourir d'un mouvement

* Robin's Tracts, vol. 2. p. 186.

uniforme

Cases of difficulty in the doctrines of moving force.

uniforme 1 pied en 1 minute, parcourra 2 pieds en 2 minutes, une infinité de pieds en une infinité égale de minutes ; il a en soi de quoi se mouvoir éternellement, quoique sa force soit finie, il faut seulement qu'il ne rencontre point d'obstacles. Je suppose cette force telle que quand il se sera mû pendant 1 minute, toûjours appliqué à un ressort qu'il fermera à la fin, et dont la base, qui répond à l'ouverture qu'il aura eûë d'abord, ait été de 1 pied, cette force soit entièrement consumée, et je suppose ensuite qu'au lieu de ce ressort on lui en donne à fermer deux consécutifs égaux à celui-là. Il ne peut les fermer sans les appliquer tous deux l'on contre l'autre, sans réduire à rien leur base commune double de la premiere, c'est-a-dire, sans parcourir un espace de 2 pieds. Or cet espace, il ne le peut parcourir qu'en 2 minutes, donc dans la primiere minute il ne peut avoir fermé qu'à de mi chacun des deux ressorts, et à la fin de la seconde il les aura entiérement fermés tous deux, et sa force sera consumée.'

Mr. Maclaurin has given, in his Treatise of Fluxions, page 431, some ingenious solutions of the problems where two or more bodies at rest are struck at the same instant by another body moving with a given velocity in a given direction. It is remarkable, however, that the consideration of the time was omitted by him in the same way that it was omitted by M. Bernoulli ; although the oversight of the latter had been pointed out by Mr. Robins fourteen years before Mr. Maclaurin published his solutions ; which appear to be defective also in the following respect. The resulting motions are first given on the supposition, that the bodies are hard and nonelastic, and from these results are deduced the motions which are supposed to result from the collision of elastic bodies. But M. D'Alembert has shown, that in all cases where the bodies which are struck are not equal to each other, and similarly situated with respect to the direction of the striking body, the supposition of hard bodies leads to erroneous results with respect to elastic ones*,

* Traitè de Dynamique, p. 234—5.

and

and it is remarkable, that the cases selected by Mr. Maclaurin are all of that description.

Cases of difficulty in the doctrines of moving force.

Far be is from me to say, that the oversights of that excellent philosopher and profound mathematician, or that the omissions or oversights of any of the distinguished men to whose works I have referred, are of much importance when compared with the numerous benefits which they have rendered to science. I only wish to show, that the principle which appears to me to be capable of general and correct application, has been condemned on insufficient grounds; and the circumstance of such a man as Maclaurin having been led to erroneous conclusions by reasoning from the supposed action of hard bodies, affords the best argument for rejecting that doctrine.

M. D'Alembert appears to have been fully sensible of the difficulties which attend the solution of problems of this description; and, from his general reasoning respecting them, as well as from the demonstrations of some of them which he has given, it is obvious that, without considering the pressure and the space through which it acts, as well as the time of its acting during the *process*, if I may so call it, of collision, the resulting velocities and directions of the bodies, after collision, cannot be determined.

I have selected the case which I have stated (as I have selected all the rest) as being the most simple of its kind; and the solution which I have offered is also simple; being derived from examining the pressures and the spaces through which they act in producing the motion of A.

The 12th example is stated for the purpose of showing, that in cases where quantity of motion, *in one direction*, forms no part of the subject to be considered, there is in the collision of non-elastic bodies, a positive loss of force, in whatever way it may be reckoned; and if that loss be estimated by examining the pressures and the spaces through which they act, a change of figure, corresponding to the force which has been expended, will be found.

The 13th case was proposed to me by my friend Mr. Dalton, to whose candid encouragement I have been much indebted in

Cases of difficulty in the doctrines of moving force.

the prosecution of this enquiry. It is stated in order to show, that the same effect is produced by the same force, whether it act by gradual pressure or by sudden percussion. If the piece of clay be placed so near to A as to touch the prism when it begins to fall, the whole impression will be produced by gradual pressure. In estimating the force in this case, a practical man thinks of nothing but the quantity of mechanical force, or the pressure into the space, necessary to raise the prism to the given height; and as the same quantity of force will always raise it to the same height, he concludes, that the same effect must always be produced by its fall, although the times in which these equal effects are produced may be very different. If, instead of a piece of clay, we place a much harder substance, a block of iron, for example, under the prism, we shall have an impression produced on the prism as well as on the block; and, unless the centre of motion be of a very permanent kind, we shall, when the block is placed near to A, have a change of figure in that centre also. But still, if all these changes of figure could be accurately measured, by the pressure and the space expended in producing each of them, their sum would be equal to the whole change of figure produced on the clay, or on any other comparatively soft substance, placed under P. There are many very complicated cases of this kind, such as the hammering and rolling of metals, which may, I apprehend, be all distinctly explained upon the same principles.

In the 14th case the same effects are produced by percussion, which, in the 5th case, are produced by gradual pressure through sensible spaces; and we must either admit, that the moving force of D (fig. 14) is greater than that of C, or conclude, that the rotatory motion is produced without force. It may be said that there is, in both cases, only the same quantity of motion in *one direction*. I must observe, however, that Sir Isaac Newton understood the *sum of the motions* of the two bodies to include the rotatory, as well as the progressive, motion. "If two globes," he says, "joined by a slender rod, revolve about their common centre of gravity with an uniform motion, while that centre moves on uniformly in a right line drawn

drawn in the plane of their circular motion, the sum of the motions of the two globes, as often as the globes are in the right line described by their common centre of gravity, will be bigger than the sum of their motions when they are in a line perpendicular to that line*." On this passage we have the following note from Dr. Horsley. "The contrary seems to be true, that the sum of the motions will be greatest when the rod connecting the revolving bodies is perpendicular to the right line along which the common centre of gravity is moved. But in either way the different quantity of that sum of motion, in these two positions of the rod, equally makes for our author's assertion. Of which, perhaps, there is yet a more striking proof in the prodigious generation of motion by the collision of elastic bodies in certain arrangements, vid. Huygens De motu corporum ex percussione." But this is obviously an oversight of the learned editor; for, if he had bestowed a little more consideration on the case, as it is distinctly stated by the illustrious author, he would not, we must presume, have given a commentary so much at variance with the text. When A is perpendicular over B, B is at rest, and A only is in motion with the velocity $2v$. The whole *quantity of motion*, when the balls are in that position, is therefore expressed in the usual way by $A \times 2v$. But when AB is in a horizontal position, the common centre of gravity of A and B is moving horizontally with the velocity $v$, and each ball is moving round that centre with the same velocity $v$. The sum of the motions, when in that position, must therefore be $\overline{A+B}.v+A.v+B.v$: and I think it will not admit of a doubt that Sir Isaac Newton understood the case in that light. But although the motion is exhibited in such various quantities according to the positions of the rod, it cannot be questioned that the *quantity of force* must remain the same under all positions of the rod. While the motion continues *uniform*, there certainly can be *no variation of the force*. It appears, therefore, (as I have before observed, p. 173) that Sir Isaac Newton understood, that unequal quantities

Cases of difficulty in the doctrines of moving force.

* Horsley's Newton, vol. 4, p. 258.

Cases of difficulty in the doctrines of moving force.

tities of motion might be derived from the same quantity of force. It must be acknowledged that from some expressions of Sir Isaac Newton, in alluding to this and some other cases, it might appear, if these expressions are taken individually, without reference to his general doctrines, that he supposed a variation of force to take place in this case. That supposition has been noticed by Mr. Bernoulli with a degree of unfortunate asperity peculiar to himself, and very inconsistent, it must be confessed, with the character by which philosophical discussions ought to be distinguished. From the context, however, it is obvious, that Sir Isaac Newton could not mean the casual expressions in question to be strictly applied as relating to variation of force in the cases which he mentions. For, if they can be so applied, they must be indiscriminately applied to cases which have no resemblance to each other. The *force* which is expended in overcoming the cohesion of pitch*, for example, can never be seriously compared with any supposed change of *force* in the case under consideration. Yet, according to Mr. Bernoulli's acceptation, Sir Isaac Newton must have meant that there was, in both cases, the same kind of variation of *force*.

If D be a non-elastic body, we shall then, indeed, have a variation of the force similar to that which takes place in the motion of the pitch. A portion of the force will be expended in producing change of figure, and the results after collision will exhibit four distinct effects of moving force, namely, a change in the progressive motion of D, a change of figure in D, a progressive motion in G, and a rotatory motion in A and B. For D will move on with the velocity $\frac{v}{2}$, and its figure will be changed; G will move on with the velocity $\frac{v}{4}$, and A and B will revolve around G with the velocity $\frac{v}{4}$. That is, one-fourth of the original force of D will remain with it after collision, one half will have been expended in changing the

* See Horsley's Newton, vol. 4, p. 259.

figure

Cases of difficulty in the doctrines of moving force.

figure of D, one-eighth will have produced the progressive motion of G, and one-eighth the rotatory motion of A and B. But if these effects must be estimated by the product of the mass into its progressive velocity, the change of figure, as well as the rotatory motion, must be left wholly unaccounted for.

If the more complicated cases of this description, where the force is neither communicated in the directions of the centres of gravity, nor in those of the centres of gyration be examined on the same principles by which I have attempted to explain the fifth case, and the case before us, it will be found, that the force expended in producing change of figure, added to that which is exhibited in the motion of the bodies after collision, will always be equal to the original force of the striking body.

---

Having stated, more fully, perhaps, than is consistent with the due limits of a paper of this kind, various opinions and explanations relating to the examples of force which I have offered to the consideration of this society, I wish to observe, that the terms pressure, force, moving force, momentum, &c. are used by different authors, and sometimes even by the same author, with various meanings. It is probable, therefore, that I may not have understood them, in all instances, in their proper, or even in their intended, meaning*. I have been careful, however, to give, in most cases, the authors' own words; and in all cases I have given such references that any mistakes of that kind may be easily detected by those who are disposed to examine the subject.

* Since page 150 was printed, I have noticed, that the following passage (line 17) "that the maximum effect must consequently be as $A \times c^2$" should be corrected thus, "that the maximum effect of a given quantity of water must consequently be as $c^2$. I wish to observe, also, that, although the reviewers admit that there is a great difference between the theoretical conclusions, and the acknowledged results of experience, they appear to consider the theory to be unexceptionable. To that I could reply only by stating, at some length, the difficulties which attend the application of the theory to practice.

That

Cases of difficulty in the doctrines of moving force.

That great misunderstandings respecting the subject under consideration, have arisen from the various senses in which the terms have been taken, must be acknowledged. But it cannot, I think, be reasonably contended, that the whole has been merely a dispute about words.

Soon after it had been shown by Huygens, that the "ascensional force of a body in motion is as the square of its velocity, that principle was extended and brought forward in a manner very unfavourable to its general reception. It was adduced by Leibnitz* as an argument against Des Cartes; and afterwards by Bernoulli and others†, as a principle which must supplant or supersede some of the leading doctrines of the Newtonian philosophy. Great opposition was naturally excited by these last pretensions; and, as it is invariably the case in intemperate controversies, the advocates on both sides were led into many inconsistencies. It soon became quite a party question, and the prejudices against one side became so strong, that if any one ventured to consider the absolute force of a body in motion to be as the square of its velocity, he was pitied or condemned as if he had lapsed into a dangerous heresy. It is to be regretted, that these prejudices, if such they are, are not yet entirely removed. For myself, I must acknowledge, that it is a matter of some concern to me, that, in consequence of the explanations which I have thought it necessary to adopt in endeavouring to understand this subject, I have, by some of my very good mathematical friends, whose favourable disposition it is my wish to conciliate, been considered more in the light of a perverse schismatic than in that of a patient enquirer; and I intreat that the two great length of this, I fear tedious, discussion may be ascribed to my desire to merit the latter rather than the former appellation.

I cannot help thinking, that if this rejected principle had been first produced, not in opposition to, but as, what I believe it really is, an extension of the Newtonian doctrine of force,

* Act. Erud. Lipsæ, 1686, p. 161.

† Works, vol. iii.

it

it would have been zealously cultivated, and might have proved highly interesting to mathematicians, as well as of essential service to practical men, in explaining those variations of force, to the useful application of which their operations are chiefly directed.

Cases of difficulty in the doctrines of moving force.

If we wish to trace the history of this measure of force to its origin, we must go back to Galileo. It was first demonstrated by him, that the spaces described by heavy bodies, from the beginning of their descent, are as the squares of the times, and as the squares of the velocities acquired in those spaces; and he first distinctly explained all the phenomena of the motions of bodies uniformly accelerated or retarded by constant forces in their simple, and likewise in their compound actions. The law of continuity appears also to have originated with him. It is most extraordinary, that both Mr. Robins, and Mr. Maclaurin have spoken of this law with great disapprobation*; and that, although it had been distinctly stated by Galileo, nearly a hundred years before the time they wrote against it, they considered it as a new and a visionary doctrine produced by Leibnitz or his followers, for the purpose of controverting the arguments which had been produced in support of the supposed collision of hard bodies. Galileo appears to have been fully sensible of the importance of the law of continuity, and to have been aware, also, of the objections which might probably be brought against it. In his first dialogue he supposes a difficulty to arise in the mind of one of his speakers, who states it thus. "Id est, quod non satis capio, cur necesse sit, ut mobile quietem deserens, et motum inclinatione naturali subiens, omnes transeat gradus præcedentis tarditatis, qui inter quemcunque certum velocitatis gradum, et statum quietis, interjecti sunt:" To which the following remarkable answer is given: "Non dixi, nec ausim dicere, naturæ ac Deo impossibile esse, velocitatem illam quam dicis, immediatè conferre: sed hoc affirmo, quod id natura de facto non præstet. Si vero præstaret, ea operatio naturæ cursum

* Robins' Tracts, p. 174-5.—Maclaurin's Account of Sir Isaac Newton's discoveries, p. 92-3.

exce-

Cases of difficulty in the doctrines of moving force.

excederet, ac proinde miraculosa foret*". This short, but comprehensive, argument contains every thing that can be urged in support of any of the principles which are termed laws of nature; and it is not easy to understand upon what grounds of experience or analogy this principle of continuity has ever been rejected.

The laws of uniformly accelerated or retarded motions having been demonstrated by Galileo, the same principle was extended by Newton to motions produced by varying forces, where the acceleration or retardation cannot be uniform; and in the 39th prop. of the first book of the Principia, it is demonstrated, that when a body is urged in one direction by a varying force, the square of the velocity which it has acquired in any given space, measured from the beginning of its motion, will be as the curvilinear area which is formed by the aggregate of the increments of the space drawn into right lines denoting the pressures exerted at each increment.

As far, therefore, as the measure of force, which is composed of the pressure into the space through which it acts, can be applied to the estimation of the forces of moving bodies, it is, properly speaking, the doctrine of Galileo and of Newton.

But we have seen, that the same principle has been still farther extended, and applied to explain the phenomena of force producing changes of figure in masses of matter.

No indications of force are more constantly presented to our notice than those which consist of mechanical changes of figure. The fabrication of every thing that is useful or convenient to us, is accomplished chiefly by the application of mechanical force to produce change of figure. The grinding of corn, the expressing of oil from seed, the sawing of timber, the hammering and rolling of metals, the driving of piles, are all examples of moving force producing changes of figure; and although, in all these cases, the effects produced are of a complicated kind, yet the moving forces by which they are

* Dialogus de Systemate Mundi. Lugduni, 1641, p. 11. This was first published at Florence in 1632.

produced

produced, may be estimated with tolerable precision. The force expended in driving piles into the earth, has been examined by many mathematicians. In this case, the whole force of a body in motion is supposed to be expended in driving the pile, and this quantity of force is understood to be as the height from which the body falls, or as the square of its velocity. But there appears to be a material inconsistency in this application of the prevailing theory. For there is, in fact, no difference in kind between this case and the 8th case which we have before examined; although, in that case, there is, according to the theory, no force expended in driving the cylinder into the ball of clay. I do not see how this inconsistency can possibly be removed, but by adopting Mr. Smeaton's explanation of the collision of non-elastic bodies. Case of Pile-[illegible] moving force.

I am aware, that many object to the comparison of changes of figure with changes of motion, as effects of force. Our knowledge of both, however, appears to be acquired by the same means. They are both produced by pressure acting through some portion of space; and there appears to be no difficulty in estimating the forces by which they are produced by the same measure.

Of all the various terms that have been adopted in explaining the phenomena which we have been examining, none has been so uniformly used with the same meaning as the word *pressure.* All our notions of force appear to be derived from *pressure,* as it is perceived by the sense of touch. By balancing and comparing all other pressures with that of gravity, we obtain a common measure of pressure. Although pressures are balanced by pressures relatively at rest, under an almost infinite variety of circumstances, their most intricate combinations are distinctly explained and estimated by the application of a small number of general principles, and upon that subject no difference of opinion exists.

If pressure be applied to a mass of matter at rest, but free to move in any direction, the mass is put in motion. But that motion of the mass implies motion of the pressure; for, unless the pressure follow and act upon the mass through some portion

Cases of difficulty in the doctrines of moving force.

portion of space, no motion can be produced. If it be clear that the motion of a mass of matter must be produced by the action of pressure through a portion of space, it is not less obvious, that the mechanical compression, or the mechanical separation of the parts of a mass of matter, must be produced by the same means; and when we speak of the resistance of inertia in one case, or of that of repulsion or cohesion in the other, we only mean, that the exertion of pressure through some portion of space is necessary to overcome the resistance in either case. Although we suppose the resistance in the different cases to proceed from different causes, we find no difference in the means by which the resistance is to be overcome; and by taking the pressure conjointly with the space through which it acts, we obtain a common measure for this description of force.

When we speak, therefore, of the *force* by which the motion or the change of figure, of a mass of matter is produced, we mean something more than simple pressure balanced by pressure relatively at rest. In the latter case we have to consider only the pressures as they are balanced by each other, without any reference to motion. But in the former case, no effect can be produced unless the pressure act through some portion of space. If the pressure be increased in the same ratio that the space through which it acts is diminished, or *vice versa*, the same effect will still be produced. The space, therefore, compensates for the pressure, and the pressure for the space; and, when taken together, they constitute a determinate measurable quantity of moving force, capable of producing effects of various kinds, but in determinate quantities, which are always proportional to the moving forces by which they are produced.

The term *force* is often indiscriminately used to signify simple pressure, as well as to denote the compound quantity of force by which the motion of a body is produced. The "force of gravity," for example, (meaning quiescent pressure) and the "force of a body in motion," are very common expressions. But these two descriptions of force are as different in kind, as lines

lines are different from surfaces, or surfaces from solids; and they have been distinguished by various authors by different terms. From the following proposition it appears, that Galileo applied the same meaning to *impetus* which was afterwards applied by Huygens to *ascensional force*. "Mobile grave decendendo acquirit eum *impetum*, qui illi ad eandem altitudinem reducendo sufficiat*." Cases of difficulty in the doctrines of moving force.

Leibnitz and his followers adopted the distinctive terms, *vis mortua* and *vis viva*. Dr. Wollaston prefers *impetus* to *vis viva*, but he sometimes uses *energy* in the same sense. The Edinburgh reviewers approve of Dr. Wollaston's application of the term *impetus*; but they propose to apply the same meaning to *energy*, which is applied by Sir Isaac Newton to *vis impressa*, namely, the pressure multiplied into the time of its action.

Mr. Smeaton uses the term *mechanic power* to express the product of the pressure into the space through which it acts, or the product of the mass into the square of its velocity. In his definition of power (which I have quoted at page 129) he refers only to its effects in producing motion. But we have seen, that he understands the same measure to be the proper one, whether the force be expended in producing motion or change of figure, and he concludes, that the effects of force "cannot be so easily, distinctly, and fundamentally compared, as by having recourse to the common measure, viz. mechanic power†."

If this principle be capable of such general application, it is desirable that it should be denoted by a distinct term, in order to obviate ambiguity or misapprehension. The compound term *moving force* has been commonly applied, by various authors, to signify the action of moving pressure, as distinguished from quiescent pressure; and, from its general use in this acceptation, I have been induced to adopt it.

It is sometimes, indeed, used for *motive force*, or the pressure uncombined with time or with the space through which

* Dialo. de Syst. Mund. p. 12.

† Phil. Trans. 1776, p 473.

it

Cases of difficulty in the doctrines of moving force

it acts. But the two terms need not be confounded, and if *moving force* were defined to be "moving pressure producing change of velocity, or change of figure in masses of matter," it could not be easily misunderstood. For, if the *moving force* be estimated by the changes which it produces, the space through which the pressure acts, as well as the pressure, must be taken into the account. In the above definition it is necessary to adopt the expression "change of velocity," in preference to "change of motion;" because change of direction is included in change of motion, and change of direction cannot be estimated by the pressure combined with the space without reference to the time. The centripetal force which retains a body in a circular orbit, is similar to quiescent pressure; the pressure at the centre moves through no space, and therefore there is no change of velocity; but if the revolving body approach or recede from the centre, any given space, the pressure moves through the same portion of space, and a corresponding change of velocity is produced Excepting change of direction, however, the above definition and measure of *moving force* apply to every case of moving pressure of which we have any experience.

The *pressure* taken together with the *time* of its *direct* action, bears a constant relation to an important class of the phenomena of moving force producing motion in masses of matter. But when the pressure is applied indirectly by levers, or other means, or when a change of figure is produced, the velocity of the pressure being different from that of the mass which is moved, this relation is no longer preserved. In cases of that description, the sum of the changes produced by the moving force, is not in any constant ratio to the time of its action. If this statement be correct, the relation between the effects of a moving force and the time of its action, cannot be reduced to a general formula. It can only be considered as an individual character or property of one class of the phenomena of moving force, a property of great importance, no doubt, but still not a general property. The *duration*, therefore, of a moving force

force cannot be taken generally as an element in the estimation of its quantity. Cases of difficulty in the doctrines of moving force.

If we attempt to estimate some moving forces by their duration, and others by the spaces through which the pressure acts,—according to particular circumstances which may appear to be more favourable to the application of one measure than the other; we cannot avoid the inconsistency of sometimes concluding that a given quantity of moving force may be considered greater or less, according to the nature of the effect it is intended to produce.

*(To be concluded.)*

---

## III.

*Additional Remarks on the state in which Alcohol exists in Fermented Liquors. By* William Thomas Brande, *Esq. F. R. S. Philosophical Transactions,* 1813.

THE experiments and observations contained in this paper, are intended as supplementary to a communication on the same subject, which the Royal Society has done me the honour to insert in the Philosophical Transactions for the year 1811*. State of alcohol in fermented liquors.

On that occasion, I endeavoured to refute the commonly received opinion respecting the *production* of alcohol during the distillation of fermented liquors, by shewing, that the results of the process are not affected by a variation of temperature equal to twenty degrees of Fahrenheit's scale; that is, that a similar quantity of alcohol is afforded by distilling wine at 180° and at 200°.

I also conceived that any new arrangement of the ultimate elements of the wine, which could have given rise to the formation of alcohol, would have been attended with other symptoms of decomposition, that carbon would have been deposited,

* Page 337.

or

State of alcohol in fermented liquors.

or carbonic acid evolved, which in the experiments alluded to, was not the case. Upon such grounds I ventured to conclude, that the relative quantity of alcohol in wines, might be estimated by submitting them to a careful distillation, and by ascertaining the specific gravity of the distilled liquor with the precautions which I have formerly described.

This conclusion may be objected to, by supposing that the lowest temperature, at which the distillations were performed, was sufficient for the formation of alcohol from the elements existing in the wine ; but it is not easy to conceive how this should happen, without some of those other changes which I have just noticed.

It has been stated, in my former paper, that the separation of alcohol from wine, by the addition of subcarbonate of potash, is prevented by the combination of the alkaline salt with the colouring-extractive, and acid contained in the liquor. I have also shortly noticed some unsuccessful attempts to separate these substances by other means than distillation.

In prosecuting the inquiry, this difficulty has been surmounted, and I shall proceed to shew, that alcohol may be separated from wine without the intervention of heat, and that the proportion thus afforded is equal to that yielded by distillation.

When the acetate*, or subacetate† of lead, or the subnitrate of tin‡ are added to wine, a dense insoluble precipitate is quickly formed, consisting of a combination of the metallic oxide, with the acid and colouring-extractive matter of the wine, and when this is separated by filtration, a colourless fluid is obtained, containing alcohol, water, and a portion of the

* Sugar of lead.

† Formed by boiling two parts of sugar of lead with one of finely powdered litharge, in six parts of water. The solution should be preserved in well closed phials, as it is rapidly decomposed by attracting carbonic acid from the atmosphere. Even while hot, a portion of carbonate of lead is formed in it.

‡ Prepared by dissolving protoxide of tin in cold dilute nitric acid.

e acid of the metallic salt, provided the latter has not been ided in excess, in which case a part remains undecomposed. State of alcohol in fermented liquors.

The acetate of lead and the subnitrate of tin produce the esired effect of separating the colouring and acid matters, in ie greater number of instances, but they are less rapid and erfect in their action, and not so generally applicable as the ibacetate of lead*, which is the substance that I commonly nployed.

The following experiment was made with a view to ascertain ie effect of this salt.

Twenty measures of alcohol, specific gravity ,82500, were iixed with eighty measures of distilled water coloured with gwood, and rendered slightly acid by supertartrate of potsh. Four measures of a concentrated solution of the subaceite of lead were added to this mixture, and the whole poured pon a filter. A precipitate was thus collected of a deep purple olour, which appeared to consist of oxide of lead combined vith tartaric acid and the colouring-extractive matter.

The filtered liquor was perfectly transparent and colourless, nd afforded, on the addition of subcarbonate of potash, 19,5 ieasures of alcohol†.

Finding

* The effect of this salt upon colouring matter, was first pointed out ) be my Mr. E. M. Noble of Chelsea.

† Pure subcarbonate of potash, obtained by igniting the carbonate. as employed in these experiments. I found that about 19,5 parts of cohol were separated in the course of four hours, by the addition of ) parts of the subcarbonate to a mixture of 20 parts of alcohol by easure with 80 of distilled water, and that no further separation took ace. The alcohol is always slightly alkaline, probably from containing small portion of the solution of subcarbonate, or of pure soda, but as is did not interfere with the object of the experiment, it was not partiilarly attended to.

When the subcarbonate was added to a mixture of four parts by easure of alcohol with 96 of water, no separation was effected.—A ixture containing 8 per cent. of alcohol afforded about 7 parts—one ontaining 16 per cent. about 15,5, and where the proportion of alcohol ceeded 16 per cent. the quantity, indicated by the action of the subirbonate, was always within 0,5 per cent. of the real proportion con-

State of alcohol in fermented liquors.

Finding that the separation of alcohol by subcarbonate of potash from mixtures of spirit and water, was nearly complete, and that colouring extractive matter, and tartaric acid, might be removed from such mixtures by the subacetate of lead, I proceeded to examine wine by such modes of analysis.

The following results were obtained by these, and other comparative experiments.

1. One part by measure of a concentrated solution of subacetate of lead, was added to eight measures of common port wine: the mixture having been agitated for a few minutes, was poured upon a filter.—The filtrated liquor was perfectly colourless, and the addition of dry subcarbonate of potash effected a rapid separation of alcohol*.

100 measures of the wine thus treated, afforded 22,5 measures of alcohol.

2. Eight ounces of the wine employed in the last experiment, were distilled in glass vessels, as described in my former paper.—The specific gravity of the distilled liquor at the temperature of 60° was 0,97530, which indicates 22,30 per cent. by measure of alcohol of the specific gravity of ,8250.

3 Eight ounces of the same wine were introduced into a retort placed in a sand heat, and the process of distillation was stopped when six ounces had passed over into the receiver. After the vessels were completely cooled, the portion in the receiver was added to the residuum in the retort. The specific gravity of this mixture (ascertained with proper precautions) was ,9884, that of the original wine = 0,9883†.

tained in the mixture. So that in the examination of wines containing less than 12 per cent. of alcohol, the method described in the text is somewhat exceptionable. The above experiments were made in glass tubes varying in diameter from 05, inch to 2 inches, and accurately graduated into 100 parts.

* When any excess of the subacetate had been employed, a portion of carbonate of lead was thrown down; but this did not interfere with the subsequent separation of the alcohol.

† This experiment was suggested in the Edinburgh Review for November, 1811.

When

When care was taken to prevent the escape of vapour, no hange of specific gravity was produced in the wine by three epetitions of the above process.

State of alcohol in fermented liquors.

Similar experiments were repeated upon Madeira, Sherry, laret, and Vin de grave, wines differing in the relative proortions of alcohol, colouring matter, and acid, which they ontain, and the results were as decisive; so that I conceive is amply proved, by experimental evidence, that no alcohol *formed* during the distillation of wines, and that the whole uantity found, after distillation, pre-existed in the fermented quor.

It has been frequently asserted, that a mixture of alcohol nd water, in the proportions I have stated them to exist in ine, would be much more effectual in producing intoxication, nd the general bad effects of spirituous liquors, than a similar uantity of the wine itself. But this is true to a very limited ktent only: when brandy is added to water, it is some time efore the two liquids perfectly combine, and with alcohol this more remarkably the case, and these mixtures are warmer the taste, and more heating, if taken in this state of imperct union, than when sufficient time has been allowed for their erfect mutual penetration.

I have also ascertained that distilled port wine tastes stronger, nd is more heating than the wine in its original state, and that ese qualities are impaired, and the wine reduced nearly to its iginal flavour, by the addition of its acid and extractive atter. With claret, and some other wines, containing less cohol and more acid than port, these circumstances are more adily perceived; and lastly, if the residuum afforded by the stillation of 100 parts of port wine, be added to 22 parts of cohol and 88 of water (in a state of perfect combination,) the ixture is precisely analogous in its intoxicating effects to port ine of an equal strength.

In the table annexed to my former paper, it appears that the erage quantity of alcohol contained in port wine amounts 23,48 per cent.; but two of the wines there alluded to are ronger than any I have since met with, and were, at that

 time,

State of alcohol in fermented liquors.

time, sent to me as "remarkable strong and old port." I have lately examined a number of specimens of the better kinds of port wine in common use, and the results of these experiments lead me to place the average strength at 22 per cent. of alcohol by measure.

A port wine procured for me by Dr. Baillie, and to which no brandy had been added, afforded 21,40 per cent. of alcohol: another specimen of a similar description, put into my hands by an Oporto merchant, contained only 19 per cent.; it is the weakest port wine I have met with.

The other results given in the table, agree perfectly with those of subsequent and more extended experiments.

---

## IV.

*Letter from Mrs.* Agnes Ibbetson, *shewing that the Spiral Wire is the causes of all Motions in Plants.*

*To Mr. Nicholson.*

SIR,

Introduction to the letter.

SOME late discoveries which I have been so fortunate as to make by means of the compound microscope, have so completely substantiated the evidence before adduced, to prove that the spiral wire is the *cause* of all motion in plants, that I shall now venture to collect all the facts into one general view, and thus give a complete idea of the mechanical force in plants, their various kinds of motion, (little known) and the mechanism by which they are directed; the consequences of that motion, and the cause of its duration and cessation; and this description being accompanied by drawings exactly copied from plants dissected for the purpose, so as to lay bare the muscles, and shew the direction of their levers and pulleys, (for such they may properly be called) will render the whole, I flatter myself, so plain and evident, as to banish doubt from the mind, and prove the truth of two general botanical propositions I have long

long been labouring to shew: viz. that the spiral wire is the cause of all motion in plants; and that there is no such thing as *perspiration in plants.*

That the regular motion in plants is little, or not at all known, is but too true, for to examine it, requires a degree of attention few will give, and a constant watching of plants that scarcely any will bestow; but the most regular instrument is not more exact in its daily motion than the leaves of most trees, especially those which have long peduncles: the necessities of each leaf are a certain portion of light without which it decays: there is not any thing (darkness excepted) which so soon destroys it, as being in too close contact with other leaves; nature has, therefore, bestowed two gatherers for the regulation of its motion, one adjoining the stem, the other the leaf, and at the termination of each gatherer, is placed a ball on which the peduncle or leaf turns as on a pivot, which carries each nearly two thirds of a circle by means of the elongation and contraction of the spiral wire. But they do not move alike: the gatherer next the leaf moves perpendicularly, and can, therefore, raise or depress the leaf to prevent its reposing on others, or injuring those beneath it by usurping too much light: while the gatherer adjoining the stem, whose motion is horizontal, can follow the daily course of the sun; or by spreading to one side or the other, adapt itself to catch those gleams of light that pass between the different leaves and offer a favourable situation: and such is the admirable delicacy of the spiral wire, that each alteration of light, though ever so trifling, produces a new effect on the tender mechanism which governs the whole, and renders it capable of moving to that situation best fitted for it. But it is not the gatherer of the leaf alone which governs the mid-rib and side rib: each of these spires have a ball which forms their central point; in which the muscles meet, and to which they all adhere, either to make the leaf flat and straight, or to draw it into a concave or convex form, as the situation of the atmosphere propels.

Motion of plants little known.

Motion of the spiral wire.

Horizontal motion.

Concave or convex motion.

It must be observed also that there are only two moments in the existence of a leaf in which it can turn its back to the sun:

when the leaf is first formed or rather developed, it always presents its back to the light to contract the muscle, and dry the pabulum; but when this is effected it changes its posture directly, and never turns again but in a storm of wind; then the spiral being more exposed at the face of the leaf than at the back, if it retained its usual posture it would be liable to tear the mid-rib to pieces; but being much contracted by the wind, it bends inwardly into so concave a form that the spiral is greatly shaded, and not exposed to such extreme agitation: it may always be remarked, that the leaves of the new shoots are almost double the size of the old ones; this is caused by the relaxation of the spiral wire while all the pabulum of the leaf is still wet: in the elm, I have known it quite double the usual size. In shewing the various management of the leaves, I must notice that every leaf has not the same mechanical force. There appears in all the vegetable tribe a regular scale which rises, according to the plant, from the highest to the lowest mechanical power; the sensitive plants are at the top of the scale, then the diadelphian tribe, which open and shut their leaves with every variation of temperature; and so violent are the motions which the first frosts of autumn produce on the spiral in the mid-rib of the leaf in a cold morning, that they often remain half unclosed the whole day, while in the summer they are bent back many degrees beyond the horizontal ninety: it is this extreme tension of the spiral that prepares their decay, still it would not destroy the leaf stem did not the top of the oil at the same time render them more rigid, and liable to break. Next to the mechanism of the diadelphian tribe, may be classed those immense leaves that generally belong to pentandrian dygynian plants; when the leaf is very large, it is necessary to give great length and scope to the spiral wire, its force being always proportioned to the length of space it reaches to *effect its motion*—this tribe, therefore, have a sort of basket work, (see Fig. 3,) at the top of the leaf, which is twisted into various forms, and through all of which the spiral wire *passes*. In all trees and shrubs the muscles are situated in the last row of the wood, *nearest the pith*, but in all plants that rise each year from the earth, they are found

Wind a violent effect on them.

Mechanism of different leaves.

found next the alburnum, by which means they are more laid open to the influence of the seasons, and the perpetual changes of the atmosphere: this, probably, would be too exposed a situa- for the winter, but causes the vegetable tribe to be more easily moved and acted upon in *summer, autumn*, or *spring*; in all plants whatever, the spiral wire, when rising from the root, as soon as it arrives at a branch, detaches a number of these muscles, and they divide and run up each leaf and flower branch, and the quantity is admirably adapted to the size of the leaf it lifts and governs—as in the cabbage leaf, they are in sets as thick as a packthread, and have not only great strength, but art adds much to their force, for they are most admirably looped and twisted to support and catch each other, and curiously balanced in power, and fixed by means of those little green balls under which the spiral appears to run, and round which they are often wound, with many a slip knot.

Different situation of the spiral.

If the large leaves of the arctium lappa are examined, and their mid-ribs dissected, it will plainly appear that the spiral runs by impulses; which, when much agitated, seems to draw and stop like the moment-hand of a watch. But this is only when a storm or quick changes of the atmosphere produce violent agitation; then the motion is indeed excessive, and in starts; this has been observed by many in the mimosa sensitiva, for when the leaves are made to change their position it is always with a jerk—and the knots formed in most spiral wire admirably accounts for this—indeed the motion of vegetables is all of this kind, though in a greater or lesser degree, according to the number of balls and knots they possess. This is only to be known by those who will take the trouble acutely to observe and *watch* plants; but is best seen if the spirals are taken out of their cases, and exposed naked to the atmosphere: then they can be compared only to a riggling worm, and the spiral would be taken for one if placed where the air could reach it; it is really beautiful, if along piece is obtained, to see it wind and start as the various insensible changes of the atmosphere affect it; it will be asked, as they are insensible, how I know it is that cause which acts; I can know it only by observing how excessive is the

Convulsions of the spiral wire.

the effect of each change of the thermometer on it, and how breathing on it even at a distance lifts it up, nay, convulses it, nearly ten minutes after one should suppose any immediate effect must be passed; if then the spiral wire can be taken out of the plant, and in this situation exhibit all the motions of the leaf; and that the refuse remaining after this dilapidation is *perfectly inert*; surely I may say that the spiral is the cause of the motion; and if this same wire is affected, in a superior degree, and answers to all the variations of the barometer and thermometer, so as to be visibly affected at the same time, and, indeed, so much more sensibly than either as to contract and dilate when no changes in those instruments are produced, but to be affected highly when any alteration of either takes place; may I not then be allowed to say that the spiral is governed by light and moisture, and is the cause of motion in plants? Heat and light, I believe, never appear without causing a great increase of moisture in the air, for I never could augment the light greatly on the solar microscope, without producing a steam on the glass. I shall not add the mechanism of more sepile leaves lest I tire my readers; but before I close the subject of leaves, I must notice that their mechanism is not in any thing more marked than their manner of decaying: the effect of the cold on the gatherers: the distortion of the leaves in those parts marked by the muscles, are so many proofs of their being the cause of the contraction. But no sooner do the hairs disappear, and the oil with them, than the very rigidity the frost brings, renders the spirals more liable to break, they crack with the first wind, and the leaves fall to the ground: but before they fall they shew (though still on the tree) that something is wanting to them, that they once possessed; they can neither turn, nor lift themselves up, but hang a dead weight, unable to screen their back from the sun's rays, or protect their face from the piercing wind, but falling on each other, increase the decay by the general contact. And this is caused by the breaking of the spiral wire, the natural consequence of its rigidity: I shall now turn to the mechanism discovered in the flowers.

Answers to the variation of the barometer and thermometer.

Mechanical force shewn in decaying.

There

Mechanical force displayed in flowers.

There are few flowers that in passing from the state of a very young bud to a seed-vessel, do not change their position five or six times. Thus, in the geranium, when the bud first arises out of the stalk, it is *necessarily upright*; but before the flower stalk has increased two inches, the flower has varied 180 degrees, and is become perpendicular downwards. Then it begins again to rise, and is a full grown bud at 90 degrees, and perfectly displays its corolla at 160 degrees. After remaining a little time in this situation, or between this and 180 degrees, it changes and declines; and, before the pericarpium has perfected its seed, it again falls to the earth*; and so necessary are each of these variations to the completion and health of the whole, that if it is impeded in turning, it dwindles away, and the seed vessel drops off. I have frequently fastened it so as to prevent its change of direction, but decay was the invariable consequence of this unnatural opposition. Most of the diadelphian flowers vary in the same manner, some genera in an exactly contrary position, many of the pentandria *very much*, and almost all the syngenesian scarcely at all. Some flowers change only three times: thus the variety is prodigious; but if they alter not in stem, the motion of their corollas is the triumph of mechanism, and of the power of the spiral wire. To watch the variation of a flower, and compare it with the barometer and thermometer, is really an experiment

Mechanism of flowers obey the clouds.

* This should serve as a direction to all drawers of flowers to imitate nature in the situation of the plant and its posture, as well as its folds, &c. &c.; since, to place a flower in a reclining manner that should be otherwise, and to paint a leaf hanging down that should be up, is to deform nature. How often have I seen a single lampanula made to face the heavens, and a lathuras to hang down. A decandria prostrata (as it turns in its perpendicular position downwards) lets its leaves fall with it, and its flowers obey the same impulse, exactly contrary to what it should appear. How exquisitely just are the last volumes of Dr. Sims's beautiful work (the Botanical Magazine) in this respect! He strictly conforms to the laws of nature; and, till they are well known, it is easy never to place a flower but as it is seen to hang; and thus banish all confusion and deformity till the posture of all plants are as well known as their beauties.

well

well worth attending to; the flowers are so infinitely the most susceptible instruments; not a change of moisture but they indicate it before the quicksilver can move; not an increase of cold but the corolla will signify it. For the flower, in its extremely delicate mechanism, keeps pace with the variation, while our instruments only *follow* it. But the corolla may be said to obey the massy vapours; for it alters with them, and will not only indicate the sun's concealment, but sometimes the passing over of each fleecy cloud. In watching a milkwart, I have seen it put on and off its little cap three times in half an hour; and it is curious, before a thunder storm, to go round with your microscope, and see the preparation your whole garden makes for it, while our instruments alter not till its effect has been impressed on the air, and has produced a general change. But the most extraordinary variation of the spiral wire alone is shown in *floods*, or preceding them. Its universal and continual trembling at this time, must have some cause well worth inquiring into: it has always, indeed, been one of the strongest reasons with me for attributing its chief management to moisture. In a great flood last spring, in three days the water rose to a prodigious height, so as to endanger one whole street in the suburbs of Exeter. At this time I placed under my eye a quantity of spiral wire taken from a geranium, and fixed it in my compound microscope; its constant trembling attracted my attention, and made me watch it very exactly. This agitation lasted a whole day; it was never still a single moment while I observed it, which I did *continually*; and its elongations and contractions were accompanied with starts which involuntarily made me imitate it. It was then, also, that I first observed the strong effect of a flood on the barometer. Mine is of the clock kind, and, I believe, a very good one. Three times in three days it rose from 28° 30′ to 29° 33′, and its quickest motion admirably tallied with the most hasty movement of the spiral wire. Surely, it is hardly possible to bring a stronger confirmation that both are directed by the same cause, and managed by the same powers. The foldings of the corolla also appear to me strong proofs of this truth. They

Effect of a thunderstorm.

Effect of great floods.

On the spiral wire.

Effect of the barometer.

Effect on the corolla.

They are always *marked* and *outlined* by the spiral wire: what, then, but its contraction can guide those plants, and preserve those folds, in the admirable order in which they are laid? View the stapelia, particularly the *campanulata*. Could any thing but this muscle preserve this washy corolla in such admirable order, and arrange its pointed tops *so even* and *exact* when *going to close*; or could any power but the spiral shut the petals of the asclepias variagata one by one, and fold them down with the same spring *observed in the muscles?* But it is in vain thus to multiply evidence; not a single motion of a leaf or corolla (if watched with care, and minutely examined) that is not more convincing than all I can write on the subject. The purpose of this letter is merely to point them out, and endeavour to prevail on botanists to seek them. I dare not hazard at once to show all that may be known of their mechanical powers, but would fain tempt a more *philosophical mechanic*, and a better chemist, to view what a plant (*well dissected*) *will present* to the astonished senses, and then to explain it better than I can.

General evidence.

I have shown, that in all plants that rise from the ground yearly, the spiral runs next the alburnum at the exterior of the wood, instead of *within it*, as in trees. Now, in all twining plants much motion is requisite, and therefore when dissected, a superabundance of the muscles are discovered. Indeed, the tendril is formed of little else; but to be assured of this, it must be taken entirely to pieces, for they are all concealed in their cases; these must be laid open ere the interior can be developed; then the tendrils will be found loaded with them. The letter given by Mr. Knight, in the Philosophical Transactions, is, in every respect, most admirable, except in the cause to which he attributes the motion, which I cannot (though with all the humility necessary in venturing to differ from so great a man) help controverting. It is his opinion, that the twisting of the tendril is caused by the pressure of the stick on the side of the tendril, which, driving the juices to the contrary side, swells the opposite part, and thus turns it. Now, if the juices could change their situation, I cannot conceive that

Effect of the spiral wire on twining plants.

Supposed cause of twisting of the tendrils.

that the increase of one part would contract the other; but I believe it will be found, that the tendril hardly ever fails to turn *before* the stick *touches it*; and I am sure, that the vessels are all longitudinal ones, and cannot, therefore, send their juices to the other side without twisting and spoiling the tendril, and therefore becoming incapable of turning; whereas all the motion explained by Mr. Knight may be effected by a spiral wire taken out of its case, and exposed to the atmosphere. It will turn to the part from which the light proceeds, change its side as the light is altered, and be equally repelled by a black, or drawn by a white paper; and as all that remains of the plant, when the spiral wire is taken from it, is perfectly inert, there can, I think, be no doubt, that it is the spiral wire that causes its motion. Examine the tendril of the cucumber when it first appears, with all its buds in their aggregate state: it never varies from the figure, fig. 5, and yet it can have been subject to no pressure, nor turned round any form. The vine runs on an inch or two straight, and then changes its direction, when, if it does not meet any thing round which to twine, it forms its first circle at the point, and then runs up into a long spiral exactly resembling the muscles of the plant, and turning *the same way.* Why does the convolvulus arvensis so trouble gardeners? because it strangles each plant; its slender stalk twines round each flower like a tendril, and the convulsive movement is caused by the quantity of spiral wire conquering the rest of the stalk, and making it obey the natural direction of the muscles. It is almost the same by the black bryony; but, if dissected with care, the tendrils will be found within the cuticle, *already curled.* But, what is still more convincing, view the backs of innumerable leaves, particularly the phascolus vulgari, on which are found a quantity of hairs of the tendril kind, attended with a sort of instrument in the shape of a bottle, of which I shall give a sketch. Not one of them surrounds an object, yet they all wind in a perpetual circle; (see fig. 7) and, as they are perfectly clear, the spiral wire may be seen winding through them, directing their figure and guiding their motion. These were formerly taken for perspiration. By

Real cause.

Various tendrils twine within the plant.

Curious tendrils taken for perspiration.

By the sketch I have given, it may easily be conceived how perfectly unlike they are to bubbles of water; and, indeed, all that were taken for that secretion were as different in form as various in figure.

Twisted by the muscles.

I may now turn to my last argument, which embraces also the perspiration of plants. I have said, that the spiral wire is formed of a middle vessel, which is partly filled with oil, which humects and lubricates the wire to prevent its being injured by friction; but from whence could that oil come? Oil is certainly to be found in many plants, but completely pure and adapted for the purpose. I had not, indeed, the smallest idea of finding it; conceive my transport at the discovery. Observing in the gatherers of the rose, the maple, and the acacia, some vessels that run directly from the hairs at the exterior of the gatherer to the spiral wire in the middle of it, I was most curious to see what liquid they conveyed, as I had generally observed, that the vessel passing from these instruments merely carried the juices within the cuticle, and thus to the wood where the liquid seemed to mix with the sap, or sometimes stopped in the bark. But I had never noticed any that pierced to the middle of the gatherer. What, then, could they convey? It was oil, pure oil, which was received by a most curious and pointed vessel like fig. 6, apparently received from the atmosphere, and entering the hair full of a clear liquid, which it seemed, by its additional weight, to force down through the different valves into the interior of the plant. I do not suppose it is oil which is first received into the instrument from the air, but perhaps only a dense vapour or matter, which having, in some of its parts, a stronger affinity with the liquid already in the hair on its entrance, is decomposed, the part strongly attracted passes to the liquid below, while the oil remains, and, pressing down the second valve, enters the interstices pure and unmixed. This is conjecture only, but it is assisted by the appearance of the hair. For the first division is cloudy and mixed, while the oil below is perfectly clear, and thus continues passing down from valve to valve, forcing the liquid before it till they both enter the plant, and leave the

Formation of oil in the spiral wire.

Vessels passing into the gatherers.

Filled with pure oil.

Formation of the oil.

Extreme beauty and clearness of the hairs.

interior

interior of the hair, so smeared and dirty, so like a glass that has had the oil within it, that it is impossible for a moment to doubt its identity. But, if the smallest unbelief with respect to the nature of the fluid can remain after seeing it, which I should conceive impossible, keep it in a dry situation for a day, and it will arrest the oil, make it grow fœtid and black, it will smear and dirty the hair within as it disappears and enters the plant, and leave no doubt on the mind. I now find that they are to be discovered in almost every plant: but in the melon and cucumber they are best seen, and most beautiful; for the oil is so very clear, and the hairs so very transparent, that it is impossible to view them without astonishment and admiration. But it is not oil alone that is thus administered; many liquids and juices are poured into the plant by this means; innumerable combinations formed, if we may judge by the variety of colours a single vegetable will often protrude. And it was all these that were taken for the perspiration! Does it not prove how greatly the philosophers of the last age were mistaken for want of applying a *microscope* to the objects they beheld? Too ready to form a system, instead of to enlighten a fact, they went on building edifice on edifice, without considering that their foundation was not secure. But it is difficult for great minds to apply to minute objects. That all they called perspiration are figures of different forms, certainly intended to draw moisture *to* the vegetable, rather than give it *out*: for that a plant should be capable of yielding oxigen all day, and water also in such quantities, always appeared to me against common sense; since it had besides to nourish itself, and to form all the various combinations necessary to its existence. But when we contemplate the reality of its formation, behold it not only receiving from the root that quantity of sap required to form its ever-increasing wood, with innumerable glutinous bladders protruded round the root to assist in creating the blood of the plant; when we see the instruments covering the cuticle of the leaves and stems, not more various in their form than beautiful in their colours, announcing, by the many juices they bring to their assistance, the variety

Mistake in supposing them perspiration.

Strange system.

Various combinations required for the support of the plant.

variety of gases they produce, then, indeed, we see a more rational system; where so many combinations must be required, where such variegations are discovered, we behold the exact preparation for them; instead of a constant flowing of liquid without use, and without limit, fit only to weaken the plant, they receive from the atmosphere a never-ceasing *repletion* of *matter*; and they give us, in return, oxigen, which purifies the air we breathe, and bestows health and vigour on every living being.

I am, Sir,

Your obliged Servant,

AGNES IBBETSON.

---

*Reference to the Plates IV. and V.*

Fig. 1. A view of the manner in which one of the lotuses is fastened to the stem by the gatherers of the leaf.

Fig. 1. The same subject reduced.

Fig. 2. The manner in which a branch is always fastened to the stem by the spiral wire over the wood, and under the bark; its appearance when the bark is stripped off.

Fig. 3. Manner of twisting of the spiral wire to lengthen it out at the top of the leaf.

Fig. 4. View of the gatherer, to shew how the hairs containing oil are fastened on, and how they communicate with the spiral wire.

Fig. 5. Tendril of the cucumber before it twines round any thing.

Fig. 6. One of the hairs greatly magnified which *holds the oil* at AA, the gatherer.

Fig. 7. The tendrils which cover the *leaf*, with the bottles drawing moisture at their *pointed ends* from the atmosphere.

Fig. 8. The spiral wire, showing the middle B. which contains the oil.

METE-

## III.

# METEOROLOGICAL JOURNAL.

| 1813. | Wind. | Max. | Min. | Med. | Max. | Min. | Med. | Evap. | Rain. |
|---|---|---|---|---|---|---|---|---|---|
| 9th Mo. | | | | | | | | | |
| Sept. 17 | S E | 30·22 | 30·11 | 30·165 | 72 | 56 | 64·0 | — | |
| 18 | S E | 30·11 | 30·01 | 30·060 | 70 | 46 | 58·0 | — | |
| 19 | E | 30·01 | 29·92 | 29·965 | 72 | 43 | 57·5 | — | |
| 20 | N E | 29·90 | 29·89 | 29·895 | 66 | 45 | 55·5 | — | |
| 21 | N E | 29·91 | 29·87 | 29·890 | 70 | 50 | 60·0 | ·29 | — |
| 22 | N | 29·96 | 29·90 | 29·930 | 68 | 48 | 58·0 | — | 6 |
| 23 | N E | 30·01 | 29·96 | 29·985 | 64 | 54 | 59·0 | — | ·14 |
| 24 | N E | 30·11 | 30·01 | 30·060 | 66 | 47 | 56·5 | — | — |
| 25 | N E | 30·11 | 30·03 | 30·070 | 62 | 51 | 56·5 | ·20 | |
| 26 | N E | 30·03 | 30·01 | 30·020 | 67 | 47 | 57·0 | — | |
| 27 | E | 30·01 | 29·94 | 29·975 | 66 | 44 | 55·0 | — | |
| 28 | N E | — | 29·94 | — | 60 | 47 | 53·5 | — | — |
| 29 | N E | 30·12 | — | — | 62 | 47 | 54·5 | ·21 | |
| 30 | N E | 30·12 | 29·84 | 29·980 | 62 | 40 | 51·0 | — | |
| 10th Mo. | | | | | | | | | |
| Oct. 1 | N E | 29·84 | 29·68 | 29·760 | 61 | 41 | 51·0 | — | |
| 2 | N E | 29·70 | 29·67 | 29·685 | 59 | 49 | 54·0 | — | — |
| 3 | S E | 29·80 | 29·70 | 29·750 | 62 | 48 | 55·0 | ·21 | — |
| 4 | S E | — | — | — | — | — | — | — | — |
| 5 | W | 29·78 | 29·65 | 29·715 | 66 | 54 | 60·0 | — | |
| 6 | S W | 29·80 | 29·77 | 29·785 | 66 | 52 | 59·0 | — | |
| 7 | S W | 29·52 | 29·47 | 29·495 | 65 | 51 | 58·0 | — | — |
| 8 | W | 29·73 | 29·46 | 29·595 | 60 | 54 | 57·0 | — | 2·50 |
| 9 | Var. | 29·43 | 29·42 | 29·425 | 61 | 49 | 55·0 | — | |
| 10 | S W | 29·44 | 29·15 | 29·295 | 65 | 45 | 55·0 | — | 4 |
| 11 | W | 29·46 | 28·93 | 29·195 | 60 | 48 | 54·0 | — | — |
| 12 | S W | 29·75 | 29·55 | 29·650 | 60 | 41 | 50·5 | — | — |
| 13 | S W | 29·55 | 29·34 | 29·445 | 58 | 52 | 55·0 | — | ·45 |
| 14 | N W | 29·62 | 29·52 | 29·570 | 58 | 34 | 46·0 | — | ·29 |
| 15 | S | 29·58 | 29·21 | 29·395 | 51 | 33 | 42·0 | ·28 | ·47 |
| | | 30·22 | 28·93 | 29·752 | 72 | 33 | 55·23 | 1·19 | 3·93 |

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

*Notes.—Ninth Mo.* 19, 20. Breezes by day: much dew. 21. Cloudy quite to sunset: a few drops of rain. 22. A breeze a. m. bringing clouds: p. m. a sudden shower: rain in the night. 23. Windy; showers. 24. Much wind. 25. Windy; cloudy. 26. Overcast a. m. clear p. m.; a luminous twilight, with cirrus and cirrocumulus. 27. Morning twilight somewhat coloured: forenoon overcast: clear p. m. and at sunset fascicular cirri, arranged from W. to E. the wind E. nearly calm: after these appearances, lightning, far to the S. E. and S. W. 28. Cloudy with a few drops. 30. A pink twilight, with dense coloured cirri. For three days past a steady N. E. breeze, with pretty much sunshine.

*Tenth Mo.* 1. Overcast a. m. wind N. After sunset cirrocumulus passing to cirrostratus; a corona round the moon, and a small *meteor* which went W. 2. Overcast most of the day; a few drops p. m. 3. Cirrus with cumulostratus: twilight opaque and orange coloured: the roads have become of late excessively dry, and the dust raised from them floats in great quantity in the air. 4. Early this morning began a steady rain, which continued till after sunset. 5. Fine day: lunar halo. 6. Cloudy. 7. A considerable storm of thunder and lightning early this morning, followed by much rain. 8. Fair, a. m. wet, p. m. 9. Fine day. 10. Wet, with a fair interval. 11. Wet a. m. fair p. m. 12. The reverse of yesterday. 14. Fair. 15. Very wet.

## RESULTS.

Prevailing Winds Easterly and drying, to the first quarter of the Moon; soon after which they became Westerly and brought much rain.

Barometer: greatest height 30·22 in.; least 28·23 in.
Mean of the period 29·752 inches.
Thermometer: greatest height 72°; least 33°;
Mean of the period, 51·23°.
Evaporation, 1·19 in. Rain 3·95 in.

The rain of the 4th instant having put the conclusion to a fair season of some weeks continuance, I availed myself of the opportunity of a journey made immediately after it to ascertain, as far as I could, its extent.

I found that it had rained from morning to night on that day all the way between London and York; also (by information from other travellers) as far North as the Tyne, and over the narrow part of the island from Cheshire to Northumberland. It having been likewise a very wet day on the South coast, I conclude that probably the whole of England was on this occasion irrigated at once, from an Atlantic current, which, during the prevalence of the Easterly breeze just before, had taken possession of the higher atmosphere; and which on that day arrived, in its progress of subsidence, near enough to the earth to part with its electricity, and displace the lower stream of air.

L. HOWARD.

TOTTENHAM,
*Tenth Month, 23d,* 1813.

V.

*Observations relative to the near and distant Sight of different Persons. By* James Ware, *Esq. F. R. S. From the Philosophical Transactions for* 1813.

(*Concluded from p.* 216.)

This imperfection is seldom alike in both eyes.

NEAR-SIGHTEDNESS is seldom alike in the two eyes, and a few cases have come under my observation, in which one eye of the same person has had a near, and the other a distant sight.

Whether the pupil in near-sighted eyes be dilated.

It has been said by Dr. Porterfield*, that the pupils of near-sighted persons are more dilated than those of others. This, however, does not accord with the observations I have made in such cases.

Probably not; but query?

It has also been commonly believed, that the size of the pupil is influenced by the distance of the object to which the attention is directed, this aperture being enlarged when the object is far off, and becoming more and more contracted as it is brought near. But though the activity of the fibres of the iris is sometimes sufficient to be visibly influenced by this circumstance, yet, in the greater number even of those cases where the dilatation and contraction of the pupil are powerfully influenced by a difference in the strength of the light, the distance of the object considered alone produces so little effect upon it as to be scarcely perceived. That it has, however, in general, some degree of power on the pupil is highly probable; and an extraordinary instance of this kind exists, at the present time, in a lady between thirty and forty years of age, the pupil of whose right eye, when she is not engaged in reading or in working with her needle, is always dilated very nearly to the rim of the cornea; but whenever she looks at a small object, nine inches from the eye, it contracts, within less than a minute, to a size nearly as small as the head of a pin. Her left pupil

Case of a contraction of the pupil when ...r objects contem...ted.

* Treatise on the eye and the manner of vision, vol. 2, p. 33.

pupil is not affected like the right; but in every degree of light and distance it is contracted rather more than is usual in other persons. The vision is not precisely alike in the two eyes, the right eye being, in a small degree, near-sighted, and receiving assistance from the first number of a concave glass, whereas the left eye derives no benefit from it. This remarkable dilatation of the pupil of the right eye was first noticed about twenty years ago, and a variety of remedies have been employed at different times with a view to correct it; but none of them have made any alteration. It should be mentioned, that, in order to produce the contraction of the pupil, the object looked at must be placed exactly nine inches from the eye; and if it be brought nearer, it has no more power to produce the contraction than if it were placed at a remoter distance. It should also be mentioned, that the continuance of the contraction of the pupil depends, in some degree, on the state of the lady's health; since, though its contraction never remains long after the attention is withdrawn from a near object, yet, whenever she is debilitated by a temporary ailment, the contraction is of much shorter duration than when her health is entire*.

Singular condition.

Dr. Wells, in his ingenious paper, published in the second part of the Transactions of the Royal Society for the year

Facts relating to the effects of dilatation of the pupil, &c.

* Several instances have come under my notice, in which the pupil of one eye has become dilated to a great degree, and has been incapable of contracting on an increase of light, whilst the pupil of the other eye has remained of its natural size. In some of these, the eye with the dilated pupil has been totally deprived of sight, the disorder answering to that of a perfect *amaurosis*; but in others, the dilatation of the pupil has only occasioned an inability to distinguish minute objects. Reading has been accomplished with difficulty, and convex glasses have afforded very little assistance. Though objects at a distance were seen with less inconvenience than those that were near, these also appeared to the affected eye much less distinct than to the other. Most of the persons to whom I allude had been debilitated, by fatigue or anxiety, before the imperfection was discovered in the sight; and in some it had been preceded by affections of the stomach and alimentary canal.

Facts relating to the effects of dilatation of the pupil, &c.

1811, has taken pains to ascertain whether the power by which the eye is adjusted to see at different distances, depends, in any degree, on the faculty in the pupil of dilating and contracting; and whether its fixed dilatation has any influence in preventing an accurate view of near objects. This last mentioned effect Dr. Wells relates to have taken place remarkably in the case of Dr. Cutting, whose pupil being fixed in a dilated state by the action of the extract of belladonna, perfect vision of a near object was removed, as the dilatation advanced, from six inches (which was the nearest distance at which Dr. Cutting could distinctly see the image of the flame of a candle reflected from the bulb of a small thermometer) to seven inches in thirty minutes, and to three feet and a half in three quarters of an hour. My eldest son, who has a very extensive range of vision, has made a similar experiment on his right eye with a similar result. Previous to the application of the belladonna, he could bring the apparent lines on an optometer (like that improved by Dr. Young from the invention of Dr. Porterfield, and described in the Philosophical Transactions for the year 1800) to meet at four inches from the eye; and, by directing his attention to a more distant point, he could prevent them from meeting till they were seven inches from the eye, after which they continued apparently united the whole length of the optometer, which was twelve inches*. He could see the image of a candle reflected from the bulb of a small thermometer, five-sixteenths of an inch in diameter, at the distance of three inches and three quarters from the eye; and he could also see the image at the distance of two feet seven inches. The belladonna produced a conspicuous dilatation of the pupil in less than an hour; after which, on viewing the apparent lines on the optometer, he was unable to

* The two lines that are perceived on looking through the slits of an optometer, cross each other precisely in the point from whence the rays of light diverge in order to be brought to a focus on the retina. And their apparent union, before and after this point, is occasioned by the unavoidable thickness of the line drawn on the optometer.

make

make them meet at a nearer distance than seven inches, or to gain a distinct image of the candle reflected by the bulb of the thermometer nearer than this distance; but he could discern it at two feet ten inches from the eye, which was three inches further than he was able to see it before the belladonna was applied. During the time of the experiment on the right eye, the left eye possessed its usual range of vision, but the sight, when both eyes were open, was rather confused, in consequence of the unequal foci of the two eyes; and it did not become clear until the pupil of the right eye recovered its usual power of contracting, which power was not acquired till the third day after the application of the belladonna.

Facts relating to the effects of dilatation of the pupil, &c.

It is remarkable that a different effect is sometimes produced on a near-sighted eye by the application of the belladonna, from that which it has on an eye that enjoys a distant sight. Dr. Wells made an experiment of this kind on a friend of his, who was near-sighted; and he informs us, in the paper above referred to, that, in this instance, the nearest point of perfect vision was moved forwards during the dilatation of the pupil, whilst its remote point remained unaltered. I have made a similar experiment on the eyes of several such persons; and though in two of these the result appeared to be similar to that which has been mentioned by Dr. Wells, yet, in the greater number, their sight, like that of those who were not myopic, has become more distant as the pupil became more dilated. In one gentleman, in whom the lines of the optometer appeared to meet at four inches and a quarter from the eye, the pupil, in half an hour after the application of the belladonna, became completely dilated; and, in consequence of this, the sight at first was confused; but both on that day, and for two days afterwards, it was evidently more distant, and the apparent lines on the optometer could not be made to meet nearer than seven inches from the eye. In a young lady, seventeen years of age, whose right eye was so near-sighted that the apparent lines on the optometer met at two inches and three quarters from the eye, these lines, when the pupil was dilated (which took place, in a small degree, in less

less than half an hour) could not be made to meet in less than three inches and a quarter; and, on the following day, the pupil being more dilated, the lines did not meet till they were at the distance of nearly four inches. In a third instance, viz. that of a lady of forty-five years of age, who had been remarkably near-sighted from her infancy, and for many years had used concave glasses of the fifteenth number (which number is ground on each side upon a tool, the radius of which is only three inches) the sight was become so confused in both eyes, that she saw nothing distinctly, and was unable to read letters of the size that are used in the printed Transactions of the Royal Society, either with or without a glass. In this case, after the pupils had been dilated by the application of the belladonna, the sight was so much improved, that she was able to read a print of the above-mentioned size, at the distance of two inches with either eye. I do not insist, however, on the present case, because, though there was not any visible opacity in the crystalline, this sometimes exists, in a small degree, without being perceptible even to an attentive observer; and it may be doubted whether the amendment in the lady's vision were not occasioned solely by the retraction of the iris from before a part of the crystalline that was not yet become opaque; it being well known, that the outer part of this lens not unfrequently retains its transparency for some time after an opacity has commenced in the part that surrounds its centre.

It is evident, that near-sightedness has no dependence on the greater or smaller degree of convexity possessed by the cornea, when this circumstance is considered alone; since the length of the axis of the eye, from the cornea to the retina, and the greater or smaller degree of convexity in the crystalline humour, must be also regarded, before the distance of accurate vision can be determined.

It is no less evident, that near-sightedness is not necessarily occasioned by a morbid protrusion of the whole eye; since some persons are born with eyes of this description, and others acquire the peculiarity, when further advanced in life, in consequence

quence of a morbid accumulation of *adeps* at the bottom of the orbit without either of them being more near-sighted than those who are free from this imperfection.

Instances of the adjustment for perfect vision having been amended in old age.

I have seen many instances in which old persons who have been long accustomed to use convex glasses of considerable power, have recovered their former sight at the advanced age of eighty or ninety years, and have then had no further need of them. Dr. Porterfield was of opinion, that in such cases the amendment is occasioned by a decay of *adeps* at the bottom of the orbit; in consequence of which the eye, from a want of the usual support behind, is brought, by the pressure of the muscles on its sides, into a kind of oval figure, in which state the retina is removed to its due focal distance from the flattened cornea. But if a morbid absorption of *adeps* at the bottom of the orbit were sufficient to restore the presbyopic to a good sight, it might be expected that a morbid accumulation of *adeps* in this part would produce a presbyopic or distant sight. This, however, has not happened in any of the cases that have come under my notice. On the contrary, in some such persons a degree of near-sightedness has been induced by the accumulation: and in others the sight, with regard to distance, has not been affected by it. It appears to me more probable, that this remarkable revolution in the sight of old persons is occasioned by an absorption of part of the vitreous humour; in consequence of which the sides of the sclerotica are pressed inward, and the axis of the eye, by this lateral pressure, is proportionably lengthened. An alteration of this kind is also sufficient to explain the reason why such aged persons retain the power of distinguishing objects at a distance, at the same time that they recover the faculty of seeing those that are near, since the lengthened axis of the eye leaves the power by which it is adjusted to see at different distances precisely in the same state in which it was before the lengthening of the axis took place*.

Although

* Dr. Young, in the paper to which I alluded in page 38, has described a great number of ingenious experiments devised by him to

Various cases of imperfection in the power of focal adjustment in the eye.

Although old persons lose the power of distinguishing correctly near objects, and require for this purpose the aid of convex glasses, they usually retain the sight of those that are distant, as well as when they were young. Instances, however, are not wanting of persons advanced in life, who require the aid of convex glasses to enable them to see near as well as distant objects. Dr. Wells is one of these. He informs us, in the paper to which I have more than once adverted, that when twenty years younger, he was able, with his left eye, to bring to a focus on the retina, pencils of rays which flowed from every distance greater than seven inches from the cornea; but at the age of fifty-five, he required not only a convex glass of six inches focus, to enable him to bring to a point on the retina rays proceeding from an object seven inches from the eye, but likewise a convex glass of thirty-six inches focus, to enable him to bring to a point parallel rays. There are also instances of young persons, who have so disproportionate a convexity of the cornea or crystalline, or of both, to the distance of these parts from the retina, that a glass of considerable convexity is required to enable them to see distinctly, not only near objects, but also those that are distant; and it is remarkable, that the same glass will enable many such persons to see both near and distant objects; thus proving, that the defect in their sight is occasioned solely by too small a convexity in one of the parts above-mentioned, and that it does not influence the power by which their eyes are adapted to see at distances variously remote. In this respect such persons differ from those who have had the crystalline humour removed by an operation; since the latter always require a glass to enable them to discern distant objects, different from that which they use to see those that are near. This circumstance, in my apprehension, affords a convincing proof that the crystalline humour is indispensably necessary to enable the eye to see at different distances. It is also worthy of remark,

The crystalline humours appears indispensable to the distant vision at different distances.

show, that the faculty of seeing at different distances is produced by a power in the crystalline humour, to become more or less convex, according as the object is more or less distant from the eye.

without

that persons who have had the crystalline humour removed, have had less power to ascertain the distance of an object when they look through a convex glass, than when they view it without this assistance; in consequence of which such persons seldom make use of glasses when they are walking: and the inconvenience of glasses is particularly experienced when they descend a flight of steps, or pass over uneven ground.

Near-sighted persons appear to have a less range of focal adjustment.

Near sighted persons do not appear to possess the same extent of vision that is enjoyed by those who have a distant sight. Being near sighted, I have repeatedly endeavoured to ascertain my own range of vision: and I find, by examining the focus of my right eye through the abovementioned optometer, that I see two converging lines, which appear to meet, with very slight variations, at the distance of three inches from the eye; and no effort I am able to make can keep these lines united further than the distance of four inches and a quarter. They then separate, and continue to diverge. With my left eye, the lines do not appear to meet nearer than four inches, and they continue united as far as five inches and a quarter, after which they also separate and diverge; so that the range of distinct vision in me does not extend further than an inch and a quarter in either eye; and within these distances I always hold a book when I read.---I find also the following rule, for determining the concavity of the glass that is best adapted for near sighted persons, to be perfectly correct with respect to myself, and, I believe, it may be safely adopted by those who, from distance or any other cause, are unable to suit themselves at the shop of an expert optician.

Rule to find the concavity of spectacles for near-sighted persons.

The rule is this. Multiply the distance at which the person reads with ease, (which, with my left or best eye, is five inches,) by that at which he wishes to read, which may be said to be twelve inches; divide the product, sixty, by seven, the difference between the two, and it leaves nearly nine inches for the focus of the concave glass that shall produce the desired effect. This is the exact concavity of the glass that I am obliged to use, to enable me to read with ease; and it answers to that sold under the name of No. 6; which, I am informed by Mr. Blunt

Blunt the optician, is a double concave glass, ground on a tool of eight inches radius on one side, and eleven inches on the other, the mean between which is very nearly nine inches. With a glass of this description I can read the smallest print, but to distinguish distant objects I am obliged to look through that, denominated No. 9, by opticians, which is ground on a tool of nine inches radius on both sides. In this respect, my eye has varied from what it was a few years ago, when I was able to distinguish both near and distant objects correctly, through No. 8. This is ground to a radius of eight inches on one side, and six inches on the other, and with it I can still read a type like that in which the Transactions of the Royal Society are printed; but am unable to distinguish through it many distant objects, which I formerly used to see distinctly.

Changes in the organ of light.

Hence it appears, that my eyes have a confined range of distinct vision, extending only to an inch, or an inch and a quarter; and that they remain nearly in the same state in which they were many years ago with regard to near objects, but have lost a part of the power which they formerly possessed, of adjusting themselves to distant ones. In this last respect, they differ from the eyes of those who have naturally a distant sight; since, as such persons advance in life, they usually retain the power of distinguishing distant objects, but lose that of seeing those that are near.

*(To be continued.)*

A

# JOURNAL

OF

# NATURAL PHILOSOPHY, CHEMISTRY,

AND

# THE ARTS.

## *SUPPLEMENT TO VOL. XXXVI.*

### ARTICLE I.

*On the Measure of Moving Force. By Mr.* PETER EWART.

*(Concluded from p.* 261.)

THIS principle of moving force may perhaps be illustrated in some degree, by comparing the phenomena of force with those of heat.—Metals and fluids having been observed to expand and contract according as their temperature is increased or diminished, it was for a long time understood that temperature was the measure of heat. After it had been proved by Dr. Black that bodies of equal temperatures contain unequal quantities of heat, it was no longer contended that temperature could be taken generally as the measure of heat. Yet temperature is a most important property of heat, and in cases where the temperature and the heat increase and diminish in the same ratio, the temperature may be used as the measure of the heat.—In cases of moving force, where the *space* described by a constant pressure, and its *duration* increase in the same ratio, the duration may be taken as the measure of the moving force. —Of absolute motion or of absolute heat, we know little,--- our researches are chiefly directed to relative heat and to relative motion.

Cases of difficulty in the doctrines of moving force.

Cases of difficulty in the doctrines of moving force.

motion.---In the estimati n of deflecting forces, the duration becomes an important element.---In investigating the phenomena of liquefaction and evaporation, temperature becomes an essential consideration. Yet there appears to be no more reason for taking duration as the general measure of moving force, than for taking temperature as the general measure of heat.

It has been shown (page 187) that if a given non-elastic body, moving with a given velocity, strike an equal non-elastic body at rest in free space, half the moving force of the striking body is expended in producing change of figure; and in the same manner it has been shown (page 197) that, when the mass of the striking body is half that of the body which is struck, two thirds of the moving force of the striking body is expended in producing change of figure.

Upon the same principles, the following general theorem is easily made out.---If any non-elastic mass A strike another non-elastic mass B at rest in free space, (the direction of the stroke passing through the centres of gravity of A and B,) the original moving force of A will be to that part of it which is expended in producing change of figure, as $A+B : B$, and to the remaining moving force of A and B after collision, as $A+B : A$*.

The practical application of this principle is exemplified in a variety of instances.---In driving piles---if the weight of the ram be very small in proportion to that of the pile, a great part

* The following is a demonstration of this. Let $v =$ the velocity of A before collision; then $\frac{Av}{A+B} =$ the velocity of A and B after collision. The *moving force* before collision will be $Av^2$, and that after collision $A+B.\left(\frac{Av}{A+B}\right)^2 = \frac{A^2}{A+B}v^2$. But these two quantities are as $1 : \frac{A}{A+B}$; hence it appears that the fractional part of the *moving force* found in the motion of the bodies after collision is $\frac{A}{A+B}$, consequently the part which is spent in producing change of figure is $\frac{B}{A+B}$.

of

of its moving force is expended in bruising the pile, and the progress of the pile into the earth is very small. The heavier the ram is in proportion to the pile, the greater is the progress of the pile, by the application of the same quantity of moving force.---On the other hand, if the object be to produce a change of figure in the substance which is struck, in hammering iron for example, if the anvil be light in proportion to the hammer, the intended effect is not produced in the same degree as when the anvil, or the mass which is struck, is heavy in proportion to the hammer which strikes it*.

Cases of difficulty in the doctrines of moving force.

If a non-elastic body strike a non-elastic machine moving with a uniform velocity (such as the float of an undershot water-wheel) the maximum effect of moving force will be communicated to the wheel when the part of it which is struck moves with half the velocity of the body which strikes it.

Let A (fig. 17) be a non-elastic soft mass, uniformly penetrable by the cylinder $c$, and moving in the direction AB with such a velocity $v$ that it would be brought to rest by driving the cylinder up to F against an immoveable obstacle---If instead of an immoveable obstacle, we suppose B to be the float of a water-wheel moving with an uniform velocity $=\frac{1}{2}v$, and to be struck by $c$ at F; in that case when B has moved through a space FH $=\frac{1}{2}$ EF, A will have arrived at G, EG being $=\frac{3}{4}$ EF, and will have lost half its velocity. In this operation $\frac{1}{4}$ of the moving force of A has been expended in changing the figure of A, $\frac{1}{4}$ remains with it when moving on with the same velocity as B, and the remaining $\frac{1}{2}$ has been expended in pressing B through the space FH, and it is easily demonstrable that if the velocity of B be either greater or less than $\frac{1}{2}v$, it will be pressed by $c$ through a space less than FH. And whether A be uniformly penetrable by $c$ or not, the same relative velocity of A and B is required in order that the greatest possible quantity of the moving force of A shall be transferred

* Examples of moving force similar to these are referred to by Mr. Leslie, in his excellent work on heat, p. 128. He explains them, however, on different principles.

 to

Cases of difficulty in the doctrines of moving force.

to B*.---It would be too much to say that this explanation may be applied to the action of water on a water-wheel, but it is remarkable that these conclusions agree very nearly with the results of Mr. Smeaton's experiments. (See page 160.)

The expenditure of moving force in overcoming the cohesion of the particles of fluids is always exhibited under very complicated circumstances; but the amount of it may in some instances be estimated with considerable exactness. When a jet of water issues from an orifice of a particular construction, it has very nearly the same velocity which a body would acquire in falling freely through a height equal to the depth of the orifice under the surface of the water.---In that case, therefore, a very small part only of the moving force is expended in changing the figure of the water before it reaches the most contracted part of the orifice.---But if the orifice be constructed so that any separation of the particles of the water from each other takes place, although they may be brought together again, and completely fill the most contracted part of the orifice, yet

* To mathematical readers it may perhaps be acceptable to have the problem in a more general form.

Problem. Given two non-elastic bodies, A and B, such that A, moving with a given velocity, $v$, shall overtake B, moving with a variable velocity, $x$, in the same right line; it is required to find $x$, such that the increase of *moving force* found in the motion of B after the stroke may be a maximum.

Solution. Let $y =$ the velocity of B after the stroke. By mechanics $\frac{Av+Bx}{A+B}=y$; and per question, $By^2-Bx^2=$maximum. That is, B. $\left(\frac{Av+Bx}{A+B}\right)^2-Bx^2=$ maximum. Reduced, $2\ Bvx-(A+2B)\,x^2=$ maximum.

In fluctions $2\ Bv\dot{x}-(A+2B)2x\dot{x}=o$, or $Bv=(A+2B)x$, & $x=\frac{B}{A+2B}v$. Q.E.I.

Cor. 1. If B be indefinitely greater than A, then its velocity after the stroke will be the same as before, & $x=\frac{1}{2}v$, which is the case in the text.

Cor. 2. If B = A, then $x = \frac{1}{3}v$.

Cor. 3. If A be indefinitely greater than B, then $x=o$.

there

there is invariably a considerable loss of moving force. In other words, a portion of the moving force is expended in producing this separation of the particles of the water; and that portion may be estimated by deducting from the whole moving force which the water would acquire in falling freely through the height of the head, that portion of moving force which is found to remain with the water after it has issued.

Cases of difficulty in the doctrines of moving force.

The following important proposition relating to this subject, is laid down by Daniel Bernoulli, in his Hydrodynamics, page 278. If a jet of water I (fig. 18) issue from the side of a vessel A, with the velocity which a body would acquire in falling freely from the surface B to C, he says the *repulsion* of the water in the opposite direction to the jet will be equal to the weight of a column of water, of which the base is equal to the section of the contracted vein, and the height equal to 2 BC.

This question respecting the amount of what has been termed the "reaction of the effluent water," derives additional interest from the circumstance of its having particularly engaged the attention of Sir Isaac Newton, and from his having given a solution of the problem in the first edition of the "Principia," which he materially altered in the succeeding editions. In the first edition (book 2d, prop. 37) he infers, that the reaction is equal to the weight of a column of water of which the base is equal to the area of the orifice, and the height equal to that of the surface of the water above the orifice. In the succeeding editions, the subject is more fully discussed in the 36th prop. of the second book, where he infers (cor. 4.) that, when the area of the surface B is indefinitely large compared with that of the orifices, the reaction is, what it was afterwards in a different manner demonstrated to be by D. Bernoulli. Sir Isaac Newton further observes, that he found, by admeasurement, the area of the orifice in a thin plate to be to that of the section of the contracted vein, at the point of its greatest contraction, in the ratio of $\sqrt{2} : 1$ nearly. He takes the reaction, therefore, to be greater than what he understood it to be when he published the first edition, in the ratio of $\sqrt{2} : 1$ nearly. He refers, however, more to experiment than to theory for a solution

Cases of difficulty in the doctrines of moving force.

solution of this question; and many valuable experiments have since been made on effluent water; yet I cannot find that the results of any direct experiments have been published which go to determine the precise amount of this reaction.

Sir Isaac Newton suggested (Principia, first edit. p. 332) a method by which the reaction may be easily measured. If the vessel be suspended like a pendulum, he observes, it will recede from the perpendicular in the opposite direction to the jet.---I have made some experiments on a vessel suspended in that manner, and in order to ascertain the reaction as accurately as possible, I made use of a balance beam furnished with a perpendicular arm of the same length as the horizontal arms, as represented at fig. 18. The scales were exactly balanced, and the end of the rod D made just to touch the side of the vessel, ---The orifice was then opened, and the water in the vessel was kept uniformly at the same height by a steam falling gently on the plate E. The scale F having been raised by the reaction of the jet, weights were put into it till it was brought exactly to the position in which it was before the orifice was opened. The diameter of the vessel was 7 inches, and the height B C exactly 3 feet. I tried orifices of various diameters from .35 to .7 of an inch. Their exact diameters were ascertained by a micrometer, and the time carefully observed in which 30lbs. of water were discharged through each orifice.

When the orifice was made in a thin plate ($\frac{1}{30}$ of an inch in thickness,) I found the re-action to be greater than Sir Isaac Newton's first conclusion, in the ratio of 1·14 to 1. There was some variation in the results of the experiments. The greatest reaction, however, was as 1·16 to 1, and the least as 1·09 to 1, which fall far short of Sir Isaac Newton's last inference. The velocity of the water at the orifice (ascertained by observing the time in which 30lbs. were discharged) was less than that which a body would acquire in falling freely from B to C, in the ratio of ·6 to 1.

I found no constant ratio to subsist between the diameter of the contracted vein and that of the orifice; and observing considerable opacity in the jet at the contracted vein, I concluded it

it to be divided into a number of different filaments, and I gave up all hopes of ascertaining the actual area of the section of the stream at that place by measuring its diameter. After repeated trials I found that when the water issued through a contracted hole, of the shape represented at G, the jet was quite transparent, and the reaction (taking the mean of 12 experiments with 4 different orifices) was less than the weight of a column of water of twice the height of the head and diameter of the smallest part of the hole, in the ratio of ·865 to 1. The least reaction was as ·85 to 1, and the greatest as ·88 to 1. By measuring the quantity of water delivered in a given time, I found the velocity of the jet, at the smallest part of the orifice, to be less than that which a body would acquire in falling freely from B to C, in the ratio of ·94 to 1. The highest ratio was as ·95 to 1, and the lowest ·89 to 1*.

Cases of difficulty in the doctrines of moving force.

From these results it appears, that when the contracted vein is not opaque, and when its velocity is nearly equal to that which is due to the head, the reaction is nearly equal to what it was concluded to be by Sir Isaac Newton and M. D. Bernoulli; and the great apparent difference between Sir Isaac Newton's first and second conclusions arises from his having been misled by some experiments to which he alludes. He says---"Per experimenta vero constat, quod quantitas aquæ, quæ, per foramen circulare in fundo vasis factum, dato tempore effluit, ea sit, quæ cum velocitate prædicta," [viz. the velocity due to the head] "non per foramen illud, sed per foramen circulare, cujus diametrum est ad diametrum foraminis illius ut 21 ad 25, eodem tempore effluere debet†." We must presume, however, that he refers to experiments made by others; for if he had made them himself, he would, no doubt, have arrived at the same results which have since been so well established by various authors,

* Although these experiments were made since this paper was read before the Society, I have taken the liberty to insert the results, because they afford a good illustration of the principle which I have endeavoured to support.

† Principia, edit. 2 lib. 2, prop. 36.

Cases of difficulty in the doctrines of moving force.

authors, and he would have stated the above ratio to be as 19·5 to 25 nearly.

But his demonstration of the reaction requires that the velocity at the contracted vein shall be equal to that which is due to the head. Now that velocity cannot be determined by measuring the imperfectly contracted vein in cases of water spouting through a hole in a thin plate.

We may safely indeed infer, that, in such cases the velocity is considerably less than what is due to the head. For, the jet being opaque, some moving force must be expended in separating the particles from each other, and the distance to which the jet from such an orifice is projected on a horizontal plane, confirms that inference. The demonstration, therefore, of the reaction can be properly applied to such cases only as those where the water, issuing through a tube properly contracted, acquires the velocity nearly which is due to the head, and in those cases the experimental results agree, as I have stated, remarkably well with the demonstration.

These results agree also with the explanations which have been given of *moving force.* If we suppose the velocity of the jet to be equal to that which is due to the head, and the vessel to move uniformly in the opposite direction CD with the same velocity, the water will be at rest as it issues.

Let $a$ represent the area of the smallest section of the orifice. Then while the vessel has moved through a space $= 2\,BC$, a quantity of water represented by $a \times 2\,BC$ has descended from B to C, and has been brought to rest. But the reaction is $= a \times 2\,BC$, and this multiplied by $2\,BC$, the space through which it has acted, gives $a \times \overline{2\,BC}|^2$ for the amount of the moving force produced, which is exactly the quantity of moving force necessary to raise the column $a \times 2\,BC$ to the height BC, and to project it with the velocity $2\,BC$. For, a moving force $= a \times 2BC \times BC$ will raise that column from C to B, and an equal moving force will generate the velocity 2BC in the same column, therefore $2a \times 2BC \times BC = a \times \overline{2\,BC}|^2$ is the whole moving force necessary to restore that column to the place and

condition

in which it was before it began to descend; and as no moving force has been expended in producing change of figure, that quantity of moving force must be found in the reaction of the water through the space which the vessel has moved while the water descended and was brought to rest. Cases of difficulty in the doctrines of moving force.

Upon the same principle an easy and simple explanation may be given, I apprehend, of the action of the hydraulic machine called Barker's mill. Let AB (fig. 19) be the perpendicular tube, and BC the horizontal arm; let $v$ express, in feet per second, the rotatory velocity of the arm at the orifice C, and let the water be supposed to issue with the velocity due to the pressure. Put $g = 16\frac{1}{12}$ feet.

If BC be a cylindrical tube, and if $q$ represent the quantity of water it contains from B to C, the centrifugal pressure upon a section of the arm at C, will be $\frac{qv^2}{4g\ BC}$; and whatever the length BC may be, the diameter remaining the same, $q$ being as BC, the centrifugal pressure at C will always be as $v^2$; and it will be equal to the pressure of a perpendicular column of water whose height in feet is $\frac{v^2}{4g}$. Then if $h$ express in feet the height AB of the water in the vertical tube, $h + \frac{v^2}{4g}$ will be the whole pressure at C; and if $a$ express in feet the area of the most contracted section of the orifice, $2a\left(h + \frac{v^2}{4g}\right)$ will express the reaction, which being multiplied by $v$, the space through which it acts in a second, gives $2av\left(h + \frac{v^2}{4g}\right)$ for the *total* moving force of the arm in a second. But a part of this moving force is expended in producing the rotatory motion of the water, and in raising it to the height $\frac{v^2}{4g}$. For, if we suppose a perpendicular tube CP to rise from the arm at C, the surface of the water in that tube would stand at P, PR being $= \frac{v^2}{4g}$. Now if instead of letting the water escape at C, it be

allowed

Cases of difficulty in the doctrines of moving force.

allowed to flow over the perpendicular tube at P, and fill another similar perpendicular tube adjoining it, and issue from an orifice at the bottom of that tube, the effect must be the same as if it issued at C, and a moving force must be expended at C, sufficient to generate the velocity $v$, in the water which passes, and also to raise it from R to P.

The pressure at C being equal to the weight of a column of water whose height is $h + \frac{v^2}{4g}$, (that is $=$ AB $+$ PR,) the velocity with which the water issues will be $\sqrt{4g\left(h+\frac{v^2}{4g}\right)}$ or $\sqrt{4gh+v^2}$. Let V express that velocity, then $a$V will express the quantity of water which passes in a second; and $2aV\frac{v^2}{4g}$ will express the moving force necessary to generate the velocity $v$, in that quantity of water, and to raise it from R to P. That quantity of moving force being deducted from the total moving force of the arm, leaves $2\,av\left(h+\frac{v^2}{4g}\right) - 2\,aV\frac{v^2}{4g}$ for the *effective* moving force of the arm in a second.

That this is the effective moving force, may be shown also in another manner, as follows:

The *absolute* velocity of the water after it has left the machine will be $V-v$, and $\frac{(V-v)^2}{4g}$ will be the head which would produce that velocity; which being multiplied by $a$V, the quantity of water delivered in a second, gives $aV\frac{(V-v)^2}{4g}$ for the moving force which remains with the water after it has left the machine.

If that be deducted from $aVh$, the whole moving force of the water, there will remain $aVh - aV\frac{(V-v)^2}{4g}$ for the *effective* moving force, which will be found to be equal to $2av\left(h+\frac{v^2}{4g}\right) - 2aV\frac{v^2}{4g}$, the *effective* moving force stated above.

The

The theory of this machine has occasionally occupied the attention of many distinguished mathematicians, and M. Euler has given two elaborate treatises on its principles in the memoirs of the Berlin Academy for 1750, p. 311, and for 1751, p. 271. Cases of difficulty in the doctrines of moving force.

His demonstrations relating to this subject are very complicated, and they do not appear to have been adopted by succeeding authors.

Mr. Waring, of America, has given quite a different theory, which has been approved of by several good writers on hydraulics. He concludes that the greatest effect will be produced when the velocity of the orifice is half that of the issuing water; and that this effect will be nearly the same as that of a well-constructed undershot water-wheel*.

The explanation which I have offered of the action of the water on this machine is different from any other that I have had an opportunity of consulting. I offer it, therefore, merely as an attempt to solve an intricate problem.

If it were possible for the water to issue with the velocity due to the pressure, it is obvious, if my explanation be right, that although a very large proportion of the moving force of the water may be communicated to the machine, moving with a moderate velocity, the maximum of effect can only be obtained by an infinite velocity. But when the water issues with a velocity which is less than what is due to the pressure, as must always be the case in practice, the velocity at which the maximum of effect is produced, may be found as follows. It should first be ascertained by experiment how near the issuing velocity can be brought to that which is due to the pressure. From the experiments which I have made, I have been led to conclude that no greater issuing velocity can possibly be obtained from a machine of this kind than what is due to ·8 of the pressure. If this conclusion be correct, it follows that, whatever may be the issuing velocity of the water, a moving force, equal to $\frac{1}{4}$ of the moving force which is necessary to generate that velocity in the water, when falling freely, is

* American Philos. Trans. vol. 3. p. 191 and 192.

expended

Cases of difficulty in the doctrines of moving force.

expended in producing change of figure; that is, in forcing the water through the tubes and through the orifice C; and if the velocity of the machine be such that PC = 5AB, the issuing velocity will be equal to the velocity of the orifice, and the whole moving force of the water in descending from A to B will be expended in producing change of figure.

For, the head due to V, the issuing velocity, will in this case be PR, which is also the head due to $v$, the velocity of the orifice. We shall therefore have $V = v$; and if CP represent the total moving force necessary to raise the water from C to P, CR = AB will represent that part of it which is expended in producing change of figure. The greatest velocity, therefore, that the orifice, when the machine meets with no resistance, can acquire, will be $\sqrt{4g \times 4h}$.

When the velocity of the orifice is less than that, V will be greater than $v$: and $V-v$, the absolute velocity of the water after it has left the machine, will be $\sqrt{\cdot 8\,(4gh+v^2)}-v$. The head or the moving force expended in producing that velocity will be $\dfrac{\overline{\sqrt{\cdot 8\,(gh+v^2)}-v}|^2}{4g}$.

The moving force expended in producing change of figure will be $\cdot 2\left(h+\dfrac{v^2}{4g}\right)$ Now when the sum of these two quantities, or $\dfrac{\overline{\sqrt{\cdot 8\,(4gh+v^2)}-v}|^2}{4g}+\cdot 2\left(h+\dfrac{v^2}{4g}\right)$, is a minimum, we shall find $v = \sqrt{2gh(\sqrt{5}-1)} = 6\cdot 3056\sqrt{h}$ for the velocity of the orifice when the machine produces a maximum of effect; and in that case the above sum becomes $= \cdot 4472h$.

We shall therefore have $h - \cdot 4472h = \cdot 5528\,h$ for the maximum of effect, supposing $h$ to represent the whole moving force of a given quantity of water descending from A to B. This effect is considerably greater than that which the same quantity of water would produce if applied to an undershot water-wheel, but less than that which it would produce if properly applied to an overshot water-wheel.

Respecting

Respecting the maximum of effect produced by machines, I wish to observe, that in the actual construction of machines it is necessary to aim at a maximum quite different from that which is usually proposed in books on the theory of mechanics. This will perhaps be best explained by examining the simple case where a given weight P, (fig. 20) connected with another W, by a string passing over the pulley F, descends vertically and raises W, without friction, from the horizontal line AC along the inclined plane AB. If we make AB : BC : : 2W : P, W will be raised to B in the least time* ; and upon this principle, the maximum of effect in machines is usually demonstrated in theory. In practice, however, the object is not merely to raise W to B in the *least time*, but to raise it with the least expenditure of *moving force*. When it is raised in the least time, P must descend through a space = AB, but when it is raised with the least moving force, P descends through a space = $\frac{1}{2}$AB only. For, if we make BD = $\frac{1}{2}$AB, and let W ascend along any concave surface DEB, of which BD is the chord, it will be raised to B by the descent of P through a space = BD, and it will be at rest when it arrives at B. This is so obvious, that it would be superfluous to give a demonstration of it. It appears then, that twice the quantity of moving force which is absolutely necessary to raise W to B, must be expended if it is to be raised by P in the least time. To determine the curve by which W will ascend from D to B in the least time, is an intricate problem, and I do not know that it has ever been solved ; but a practical approximation to it in any particular case may be easily found. A well constructed steam-engine for raising water exhibits in every stroke a practical example of the same problem. At the commencement of the stroke, a very great pressure of steam is thrown upon the piston, and this pressure is gradually diminished, so that at the end of the stroke there is a considerable preponderance in the opposite direction. In consequence of this regulated pressure of the steam,

Cases of difficulty in the doctrines of moving force.

* If the ascent be made in the least *possible* time, W must ascend not along the plane AB, but along a concave surface AGB.

Cases of difficulty in the doctrines of moving force.

steam, the motion of the machine resembles the uniform vibrations of a pendulum, and the moving force of the steam is applied to the greatest advantage.

By proceeding on the principle that when W is raised to B in the least time, the maximum of effect is produced, many erroneous conclusions have been drawn respecting the proper construction of machines. It is laid down for example, on this principle, that "In an overshot water-wheel, the machine will be in its greatest perfection, when the diameter of the wheel is two-thirds of the height of the water above the lowest point of the wheel*." But it is very well known that there would be lost, by that construction, nearly one-third of the moving force of the water, which is saved by making the wheel one-half larger in diameter, and by making its velocity much less than what is required by the above rule.

It should be borne in mind, that the mechanical effects produced by means of machines, consist, almost invariably, of changes of figure. Even when a given mass is raised with an uniform velocity to a given height, a change of figure only is produced. For, if the mass were pressed to the earth by the elastic force of a spring instead of a force of gravity, we should not hesitate to say, that a mechanical change of figure is produced when it is raised. Changes of figure of this kind being easily estimated, the raising of a given weight to a given height, has long been adopted as a convenient common measure for almost every kind of moving force. If the rule, quoted above, for the construction of an overshot water-wheel, had been tried by this measure, its fallacy would have been apparent.

Dr. Wollaston has described a case of collision and change of figure, which has been understood to prove, that the force of a body in motion may be properly estimated either by the duration of its action, or by the space through which it acts, according to the particular views which may be taken of the phenomena. C (fig. 21) is supposed to be a ball of clay, or any other soft and wholly inelastic substance, suspended at rest, but

* Gregory's Mechanics, vol. 1, p. 447.

free

free to move in any direction with the slightest impulse; the two pegs, O and P, to be similar and equal in every respect, and to meet with uniform and equal resistance in penetrating C; the weight of A to be double that of B, the velocity of A moving in the direction AC, to be half that of B, moving in the opposite direction BC, and A and B to strike their respective pegs at the same instant. The result will be as follows. C will remain unmoved, A and B will be brought to rest in the same time, and the peg P will be found to have penetrated C twice as far as it has been penetrated by O. This case appears to me to admit of the same explanation as some of those which we have already examined. It is considered by many, however, to show distinctly, that the forces of A and B are equal. If we confine our attention solely to the circumstance of C remaining at rest, we must no doubt conclude, that the opposite forces of A and B are equal; but if we attend to all the results of the experiment, we cannot consistently draw that conclusion. It has often been asserted by the advocates on both sides of this question, that we can judge of forces only by their effects; yet it has been contended by M. D'Alembert*, and by many other good writers on dynamics, that the estimation of forces by their total effects, involves a metaphysical question which ought not to be mixed with experimental investigations of physical facts. It may be safely affirmed, however, that nothing can be more strictly grounded upon experiment, than conclusions derived from the examination of mechanical changes of figure.

Cases of difficulty in the doctrines of moving force.

This term, as has been already observed, includes every change of figure which requires moving force, or pressure acting through some portion of space, to produce it. Whether it be the repulsion or the cohesion of the integrant parts of bodies, or the attraction of masses to each other, that is to be overcome, mechanical change of figure is produced; and we have seen, in various cases which have been examined, the uniform relation which subsists between determinable quantities of change of figure, and the moving forces by which they are produced. We

* Traité de Dynamique, Disc. Prélim. p. 22.

find

Cases of difficulty in the doctrines of moving force.

find by experience, that when a body in motion is retarded or brought to rest, either a change of figure is produced, or a quantity of moving force, equal to that which the body has parted with, is communicated to some other body or system of bodies. It has been supposed, indeed, that A and B, in the case stated, may be brought to rest without any change of figure being produced. That supposition, however, is contradicted by universal experience, and in point of fact we may, with as much consistency, suppose that a body may be put in motion without force, as that two bodies moving in opposite directions may destroy each other's motion without producing change of figure. It appears then, that if any metaphysical consideration has been improperly mixed with this question, it is the supposed possible existence of perfectly hard non-elastic substances. But unless we have actual proof of the existence of such substances, we can have no evidence derived from experience to justify the inference, that A and B may be brought to rest without producing change of figure. When a physical experiment of any kind is made, it is generally understood, that unless all the results be collected and examined, erroneous conclusions may be formed. If an experimenter reject some of the results which he obtains, on the supposition, that sometimes they may not occur, although in fact they constantly occur in determinate quantities, he cannot reasonably demand assent to general conclusions drawn from so partial an examination of the facts. If this reasoning be well founded, we cannot reject the consideration of the changes of figure produced by A and B; and if we have no experience of a mechanical change of figure being produced without moving force, nor of bodies destroying each other's motion without producing mechanical change of figure, we cannot, in the case before us, consistently do otherwise than estimate the absolute forces of A and B by the respective changes of figure produced by each.

I shall now conclude my observations with a simple application of the principle which I have endeavoured to support, to the resolution of compound moving forces.

If we suppose BAC (fig. 22) to be a right angle, and three strings,

strings, AB, AC, and AE, in the same plane, to be united at A; the strings AB and AC to be prolonged to a length indefinitely great, when compared with the diagram, and the end of each of the three strings to pass over a vertical pulley. If the parallelogram be completed, and if three weights *m*, *n*, and *o*, which are to each other as AD, AB, and AC respectively, be suspended by the respective strings AE, AB, and AC, they will balance each other, and the strings will coincide in direction with the diagonal and sides of the parallelogram. If the weights be set in motion, by taking from *m* an indefinitely small part of its weight, *n* and *o* will descend raising *m*, and the point of junction of the strings will move in the direction AD. When that point has arrived at D, the weight *m* will have ascended a space equal to AD, *n* will have descended a space equal to AB, and *o* will have descended a space equal to AC. The quantity of moving force, therefore, is, on one side $m.AD$, balanced on the other side by $n.AB+o.AC$; the moving force of each string being as the weight suspended to it multiplied into the space through which it has moved. So that in this case, where the parallelogram is right angled, the moving forces in the different directions are as the squares of the diagonal and the respective sides of the parallelogram.

Cases of difficulty in the doctrines of moving force.

When BAC is not a right angle, let the parallelogram be completed, and the weights suspended as before, and draw DF and DG (fig. 23) perpendiculars to AB and AC. If the weights be set in motion, the point of junction of the strings will move in the direction AD, and when that point has arrived at D, the weights *m*, *n*, and *o*, will have moved through the spaces AD, AF, and AG respectively. The moving force, therefore, is on one side $m.AD$ balanced by $n.AF+o.AG$ on the other side; or the moving forces in the different directions are respectively as the square of AD, the rectangle AB. AF, and the rectangle AC. AG.

This conclusion, however, involves the geometrical proposition, that the square of AD is equal to the sum of the rectangles AB. AF and AC. AG, a property of the triangle which is demonstrated in the first prop. of the fourth book of Pappus;

Cases of difficulty in the doctrines of moving force.

and that prop. unfolds, as he observes, a general principle, including the properties demonstrated in the I. 47, and VI. 31, of Euclid. For the following concise demonstration, I am indebted to my friend Dr. Roget. Draw BH and CI perpendiculars to AD. Then the triangles ABH and ADF being similar, AB : AD : : AH : AF. Also ACI and ADG being similar, AC : AD : : AI (=HD) : AG. From these proportions we obtain the following equations $AB.AF = AD.AH$ and $AC.AG = AD.HD$, which being added together, give $AB.AF + AC.AG = AD.AH + AD.HD = AD.(AH+HD) = AD^2$*.

Various other interesting and useful examples might be given of the application of the measure of moving force, which consists of the pressure multiplied into the space through which it acts; but I believe I have already exceeded the proper limits of a dissertation of this kind, and doubtful as I must be of the favourable reception of the reasoning which I have adopted, I am more disposed to curtail than to lengthen it.

By way of recapitulation, however, I wish briefly to observe, that we appear to derive all our notions of force from pressure as it is perceived by the sense of touch, and that in all cases where neither the velocity nor the figure of the body pressed is changed by the pressure, we have only simple pressure balanced by pressure, the various combinations of which have long ago been explained and demonstrated in the most satisfactory manner.

But in all cases where either the velocity or the figure of the body pressed is changed by the pressure, we have examples of moving force, which may be properly represented by a rectangle; of which the pressure forms one side, and the space, through which it acts, the other side : and however various and complicated the changes of velocity and of figure may appear, they must all be derived from determinate quantities of moving force. We may have changes of rectilineal velocity in various directions, changes of rotatory velocity, and changes of figure,

* The same proposition is demonstrated in the II. 19. of Professor Leslie's Elements of Geometry.

all

*all* produced at the same time by a given quantity of moving force; and it is certainly a desirable object to determine what portion of that quantity has been expended in producing each of these different effects. I have endeavoured to show that all these changes may be distinctly explained and estimated, by examining the pressure and the space through which it acts in producing them.

Cases of difficulty in the doctrines of moving force.

In objecting to the opinions of many eminent writers on mechanics, I have ventured much. Although this has not been done inconsiderately, I am sensible there are in the arrangement of my arguments some faults, and others which have escaped my observation, will no doubt occur to the reader. But if my endeavours to make this essay more free from imperfections than it is, had been successful, it would still be unreasonable to expect it to obtain more attention than has been paid to the arguments of the illustrious men who have preceded me in the same track of investigation. If I have succeeded so far only as to show, that the prevailing doctrines of force, especially in their application to practical purposes, involve some difficulties which are unexplained; and if I have offered any inducement to men of science to re-examine this question, my chief object will in a great measure be accomplished.

---

## II.

### *On a new phenomenon of the Electric Column, produced by the Sun-rays.*

*To William Nicholson, Esq.*

*Windsor, Nov.* 8, 1813.

SIR,

SINCE the date of my paper published in your number of the 1st of October, I have found a new and interesting phenomenon produced in the electric column, by the sun's rays falling

falling upon it, which I shall describe, after having explained why I have observed it but lately.

Former apparatus referred to.

In my paper published in your Journal for October, 1812, I have given the figure of an apparatus connected with my first electric column, indicating, by the difference in the frequency of the strikings of a pendulum, a remarkable effect of the difference in the electric state of the ambient air; for which reason I added to the first appellation of that instrument, that of aërial electroscope.

Exposition of the same.

I intended to follow the same observations in an easy manner, by placing the column on a table separated from that on which I am differently employed. The apparatus of the pendulum requiring to be steady for its original function, I was induced to fix it, with the column, on that table; and for reasons of convenience, I fixed it in a situation that prevented me from observing the phenomenon which is now my object.

Larger apparatus according to Mr. Singer's method:

In the course of the summer I have constructed a new electric column of 1000 pairs of zinc plates and pieces of Dutch-gilt paper about 2 inches square, with the addition made by Mr. Singer of a loose piece of paper between the pairs, the importance of which I have explained in my former paper. The power of this column is so much greater than that of the first, that it produces the oscillation of a pendulum consisting of a gilded pith-ball, 4-10ths of an inch in diameter, suspended by a thin silver wire 5 inches long, communicating, like the former, with the positive extremity of the column: the gilded ball, thus suspended, comes down between the same large brass balls, one in connection with the positive extremity of the column, the other with the negative.

with a different exposure.

This apparatus being finished, it was to have been fixed on the same table; but as it promised more interesting observations, I placed it opposite to my window, at a certain distance from it. The frequency of strikings was at first too great to be regularly counted; but having means of changing the distance between the large balls, I found by trials, a distance at which the strikings continue every day, the whole of the 24 hours.

Increased fre-

This phenomenon of the column interested me, as affording a new

a new kind of physical perpetuum mobile, with the circumstance that it exhibits almost to the eye the electro-motion in the column, explained in my former paper, by comparing the pendulum to a bucket which, when filled with electric fluid by its communication with the positive extremity of the column, transport that fluid to the negative; and, by the property of the column, producing again its motion towards the positive extremity, there is thus a perpetual circulation of the electric fluid by the alternate strikings of the pendulum, which are only more or less rapid in different days, and different parts of each day, according to the electric state of the ambient air; and these changes were at first the only object which I had in view in continuing to observe them; but afterward a new phenomenon attracted my attention. quency of oscillations,

My window is turned towards the south, and the distance at which my apparatus is from it, prevented the sun from shining on the table in summer, because as it was too high; and this continued till the beginning of October; but at last its rays fell, not only on the table, but on the column itself; and then I observed so great an increase in the frequency of the strikings, that it became a new object of observation, which, however, is not often possible, at least in my room, because, in this season, the sky is frequently cloudy, or so hazy as to weaken the rays of the sun; but during many days I have been able to make the following correspondent observations. occasioned by the solar light.

Before the sun shines on the column, when, however, its rays begin to fall on the table, I count the number of strikings in one minute: they never exceed 15, and very often they are only 12. But when the sun's rays fall on the column itself, I have often counted 25; then when the sun retires, the number diminishes and returns to that of the morning.

This phenomenon might be supposed to depend on the differences of heat produced by the sun's rays in the column; which is a question of natural philosophy deserving notice, as it relates to the important object of the effects of the sun's rays in the atmosphere; of which I shall speak, after having removed that explanation by a direct experiment on the column itself, related The light does not operate as heat;

related in my former paper, which proves that the increase of heat, far from increasing its power, diminishes it, by drying the papers, and thus lessening their conductive faculty. This experiment is related in my paper on Mr. Singer's column, at p. 103 of your Journal for October, in the following manner. "Having dried every part of a column, by placing them near "the fire of my chimney, when I remounted it in that state, "the electromotion had almost ceased; but having dismount- "ed it, and laid all the pieces separately on a table, where they "remained the whole night, acquiring thus the degree of "moisture of the air in the room, when I mounted it again, "the same electro-motion was restored."

but by an increased quantity of electricity.

This experiment proves, that the effect of the sun's rays on the column cannot be attributed to an increase of heat; and there remains only one explanation, that of the production of a new quantity of electric fluid by the sun's rays. This conclusion will carry me farther, as it recalls M. De Saussure's observations on the atmospheric electricity, related in the 2d vol. of my work Ideés Sur la Meteorologie, p. 411, correspondent with my observations on the progress of heat in the atmosphere in the course of the day, detailed in the same work.

M. De Saussure, to whom experimental philosophy is indebted for many important discoveries, made his observations on the atmospheric electricity with an apparatus which I had seen before I left Geneva, and which I must first describe.—He had erected, on a terrace projecting considerably from his house in front of a plain, a mast about 100 feet high, having at the top an insulated metallic rod, from which a metallic wire descended down almost to the level of the terrace, whence it passed, for insulation, through a glass tube in the wall of a summer-house, and was there connected with a pair of pith balls, moving at a proper distance from a scale, and their degrees of divergence indicated the degrees of the atmospheric electricity.

De Saussure's observations on the daily changes of atmospheric electricity.

M. De Saussure gives the general results of his observations as follows. "In winter, the season in which I have observed "most regularly the electricity in the serene air, it appears to "me that the period in which it is the weakest, is between the time

"time of the evening dew and sun rise: it then increased by "degrees, arrives, sooner or later, but always before the "middle of the day, to a certain maximum, whence it seems "gradually to decrease, till the dew falls again."

The author's observations on the atmospheric changes of heat, &c.

After having related these observations of M. de Saussure, I compared them, in the same work, with my observations on the changes of heat in the atmosphere during the same periods of the day, in clear weather. I was led to these observations in my attempt to measure the heights by the barometer, which I have related in my work, Recherches sur les modifications de l'Atmosphere. There I first proved, that the unsuccessful attempts of former experimental philosophers, which had finally produced among them the opinion, that this measurement could not be submitted to any rule, on account of unknown changes in the nature of the air, was principally owing to their not having taken notice of a cause on which greatly depends the degree of density of the air, viz. the difference of heat; and that the attempt to make this measurement would be always unsuccessful, if the actual temperature of the air was not introduced as a datum in the formula.

When I had discovered this necessary condition, it immediately reconciled together many of my observations which gave very different results at the same height on a mountain; because I could judge, from other circumstances, that the temperature was different. Therefore, with the view of accurately determining the effect of the differences of heat on the pressure of that column of air which produced the difference of height in the barometer at the foot, compared with that in certain parts of the mountain, I often observed the correspondent changes of heat and of differences of height in the barometer, from sun rise to sun set. Thus I found generally, in clear weather, that the heat increases in the atmosphere from sun rise to about 1 o'clock in the afternoon, and then diminishes gradually till sun-set.

The changes of heat and of electricity have an agreement in their periods.

This coincidence, with respect to periods, of the progress of heat in the atmosphere, and that of atmospheric electricity observed by M. De Saussure, appears to me of great importance in meteorology. Thence we learn, that the sun's rays form

form at the same times in the atmosphere, two fluids most essential in all the phenomena on our globe's surface: the fluid which produces heat, the appellation of which among natural philosophers from time immemorial, was fire, or its correspondent in all languages both ancient and modern; and the other fluid, unknown till the beginning of last century, which has been called electric fluid, from electrum, the Latin name of yellow amber, which has the property, when rubbed, to produce the motion of light bodies, as do the bodies containing more or less electric fluid than the ground.

Inferences that many natural effects may depend on properties of light, &c. emitted from the sun and hereafter to be observed.

Dr. Herschel, in his important analysis of the rays of the sun separated by the prism, has proved, that some rays beyond the red rays of the spectrum, produce heat without being visible; and Dr. Wollaston having observed the spectrum at the opposite side, has found some rays, also invisible, which produce chemical effects. These two invisible rays undoubtedly produce, in the atmosphere, some kinds of unknown fluids; and thus manifest to us a store of future discoveries, in a field where we more and more perceive the want of known causes, to account for known phenomena. Struck by the number of effects that the sun beams must produce in the atmosphere, I have represented them, in a metaphor, as a bundle of causes (in French *un faisceau de cause)* which, when more studied and by new discoveries, will disclose many mysteries in which the physical phenomena of our globe are still involved.

But we are already informed, by the correspondence of the periods of the day in which heat and electricity increase and diminish in the atmosphere, that the sun beams form in it two of the fluids the most efficient in terrestrial phenomena, fire and the electric fluid; in which these beams, being united with some other substances, lose the faculty of being perceived by our sight; as fire, being united with water in the aqueous vapour, or steam, loses the faculty of being perceived by the thermometer.

Such are the results of experiments and observations concerning the origin of these fluids, shewing how light enters into their composition; but we have also in their decomposition, a farther

farther proof that they contain it. They are both of such a nature, that they are decomposed when too much compressed, in which case some of their ingredients are manifested.

Analogical observations.

We have again on this object to direct us, the analogy between steam, fire, and the electric fluid; for as the whole of the composition of steam is known, it illustrates the correspondent phenomena of the two other fluids. Steam is composed of fire and water in an elastic form; and when it is compressed in a metallic barrel, as, for instance, in that of the air gun, in which the air compressed is always mixed with aqueous vapour, which the strong compression decomposes, the fire disengaged heats the barrel, and the water is collected on its sides and at the bottom.

Here we see the whole process, which explains, by analogy, the known phenomenon, that when a bar of iron is hammered, and thus the fire contained in its pores is compressed, its heat is increased, till at last it glows, by the decomposition of some of the particles of fire. This is the reason why the same bar, when it has cooled, cannot be made again to glow by hammering; because it cannot be farther compressed: and from some effect produced by a new combination of the matter of fire with the iron, this becomes brittle.

Electricity lost by scintillation.

Thus also, when the electric fluid becomes so dense as to spark, a part of it being decomposed, light is emitted; and I have proved directly, by an experiment related in p. 425, of the 1st vol. of my work, Ideés Sur la Meteorologie, that when the electric fluid emits sparks, its quantity is diminished. For this experiment, I used two large insulated brass disks, having comparable electrometers: the disks were so disposed as to front each other at a small distance. I made one of these disks to communicate with the rubber of an electric machine, and the other with a small prime conductor, by which means, one of the disks became positive, at the expense of the other, rendered negative to the same degree, as shewn by the electrometers, and in that state I separated the disks from the electric machine.

The experiment related.

Each of the disks had a moveable ball; and these balls were kept, by springs, at a certain distance from each other, while the

the disks were in the position above described; but the balls could be brought, by a silk thread, to meet together; and then the positive disk, by a spark, was reduced to the same electrical state as the other. If, therefore, the same quantity of electric fluid remained in the disks after the last operation, the positive compensating the defficiency of the negative, both electrometers would have been reduced to zero of their scale; or the electric state of the ground; but it was not so, both disks were negative of many degrees, as shewn by the electrometers. Being thus informed, that when the electric fluid emits light in sparks, a part of it is decomposed, we are certain that light enters into its composition.

These facts, aided by the discoveries which almost daily increase in experimental philosophy, may, in time, lead to some explanation of the new phenomenon which is here my principal object; that of the increase of power produced in the electric column by the sun's rays falling upon it; since I have proved above, that this increase of power can only be attributed to the formation of new quantity of electric fluid in the column itself; which quantity is soon distributed, through the air, to the surrounding bodies, when no more is produced in it by the sun's retreat. But this being an essential phenomenon, which would not readily be admitted by experimental philosophers, if ascertained by only one observer, I shall adduce some observations even more striking than mine, made afterward by another experimental philosopher.

Other facts by Mr. Haussman respecting the increase of electricity by the solar light.

You have mentioned, Sir, in the same number of your Journal, an Hanoverian gentleman, Mr. Haussmann, but only on account of his having undertaken the construction of my hygrometer, and published very useful hygroscopic tables. But as his attention had been engaged by this instrument from its importance in meteorology, he wanted to study at the same time the phenomena of the electric column, and by degrees he has constructed one much more powerful than mine, being composed of 10,000 pairs of plates, $1\frac{1}{2}$ inch square; and he has so settled the parts of that column on an insulating board, that the apparatus may be transported, without any disturbance, wherever

ever it is convenient for particular observations. He has also an apparatus for the pendulum; but for the purpose of his observations, it is not necessary that it strikes constantly: his ball is heavier than mine, and he only connects it to the column for particular purposes.

Having communicated to this ingenious experimental philosopher, the new phenomenon produced by the sun's rays on the column, he found in it, first, the explanation of a phenomenon which had surprised him. His column is usually kept in a room of a north aspect, where the sun shines only in the morning; and he had remarked, that till a certain part of the morning, the pendulum did not strike; but it began at once to strike very fast, then ceased; which striking and ceasing to strike corresponded, for the time, to the sun shining, and ceasing to shine on the column. Mr. Haussmann has made many observations on other phenomena produced by his column, promising to afford results deserving to be published by himself, which, therefore, I will not anticipate, but remain to my object.

Mr. Haussmann's large column afforded a result similar to that first related.

After my communication of the new phenomenon, Mr. Haussmann having found in it the explanation of the above related phenomena, he resolved to make a more complete experiment. Taking the column from his room, where the pendulum did not strike, he transported it on an outward gallery where the sun was shining: very soon the strikings were so rapid, that he could not count them; but they also soon ceased when the apparatus was carried back to the former room.

General remarks.

Thus, therefore, the production of a new quantity of electric fluid in the column, by the sun beams, is, I think, ascertained, at least to a degree deserving the attention of experimental philosophers; and I hope it will have the effect of multiplying this apparatus, and increase the number of its observers. This phenomenon certainly opens a new road of investigation in one of the most important branches of natural philosophy, that of the influence, in the phenomena of our globe, of the sun's rays, both directly, and by the already ascertained formation of

two

two of the fluids, most essential in these phenomena, the fluid which produces heat, and the electric fluid.

I consider as a circumstance promising great advantage in forwarding experimental philosophy, a correspondence now established between Mr. Singer and Mr. Haussmann, as these two experimental philosophers do not stop on the superficy of the phenomena, but having followed them as deep as the present means can permit, they are able to devise new means of exploring their still hidden parts, and thus to dispel many errors which had crept into natural philosophy, by hasty and unwarranted conclusions.

I have no doubt that this favourable change will be seconded by Mr. Singer's work, announced in the same number of your Journal as being put to the press, under the title of Elements of Electricity and Electro-chemistry; because I know, by our conversation when Mr. Singer came to Windsor, that he has followed these experiments, not only with all the known instruments, but with many of his own invention.

I have the honour to be,

Sir,

Your most obedient, humble servant,

J. A. DE LUC.

---

## III.

*Letter from W. H. Wollaston, M. D. Sec. R. S. together with a Report of Mons. Biot, of the Imperial Institute of France, upon Periscopic Spectacles.*

*To Mr. Nicholson.*

SIR,

IN the 157th number of your Journal (for February last) your correspondent Mr. Jones renewed his attack upon the periscopic construction of spectacles, maintaining, as before, that the

he principle, on which that form of glass is recommended for pectacles, is not new, though all his quotations prove, that it was unknown to the authors on whose opinion he so confidently relies, and though it evidently is not even yet rightly understood by himself.

I have hitherto thought it wholly superflous to make any answer. Those who understood the subject would certainly not expect any reply from me ; those who did not, would not be benefited by any attempts of mine at further illustration ; and to Mr. Jones himself, it is probable that my silence would be far more satisfactory than any explanation that I could give.

I do hope, however, that the following report from M. Biot, will gratify those who are best acquainted with the merits of the question by its fairness and perspicuity ; that the authority, of one so justly celebrated as a mathematician, will be received as conclusive by those who do not feel themselves competent to decide on such subjects ; and that possibly even Mr. Jones himself, if his "*duty to his professional interest**" should again impel him to write upon the subject, may at least acknowledge that a philosopher of the first eminence in France, probably writes without any prepossession liable to warp his judgment, and that he may perhaps even feel persuaded, that there must be some advantage in the periscopic construction, which he has overlooked, when one so peculiarly skilled in optical science as M. Biot gives such decided testimony to the superiority of this kind of spectacles.

I hope you will find that I have fairly translated the whole of the report ; but as it is possible that I may in some instances

* See vol. 34. p. 101.—The liberality of Mr. Jones must be acknowledged in avowing himself the champion of the professional interest against an intruder who has presumed to recommend, as an improvement, a mode of construction, which is necessarily far more costly, on account of the thickness of glass that must be employed, on account of the quantity of this glass that must be ground away by hard labour, and more especially on account of the very small number of large glasses that can be arranged by the side of each other on a surface of small radius, so as to be ground at the same time with the same tool.

have

have misinterpreted the strict meaning of the author, I beg you will refer those who may wish to see the original, to the Moniteur of the 21st of September last.

I remain,
Sir,
Your obliged and obedient Servant,
W. H. WOLLASTON.

*Nov. 20th*, 1813.

---

*Observations on a new Kind of Spectacles, invented by Dr.* WOLLASTON.

Every one knows that those whose eyes are too convex, cannot see distant objects distinctly, because the pencils of rays of light intersect each other in the eye before they reach the retina. On the contrary, those whose eyes have too little convexity, as is generally the case in old persons, cannot see with distinctness those objects that are at a short distance, because the rays converge towards a point that is beyond the retina. The former defect is remedied by the use of concave glasses, which remove the focus of rays to a greater distance; the latter is relieved by convex glasses, which have the effect of shortening the focus.

But those who have recourse to common spectacles, cannot see with distinctness any objects which are not nearly in the direction of the axes of the glasses. Objects seen remote from the centres, are distorted and confused by reason of the obliquity of the rays of the surfaces of the glass, which occasions a degree of irregular aberration. Hence, with such glasses, the view can embrace but a small number of objects at a time. The head must be moved in such a manner as to direct the axes of the glasses to each object in succession, with great inconvenience in very many instances.

It is now some years since Dr. Wollaston proposed a remedy for this defect by a very simple invention. He remarked that, since

ince the pupil of the eye is of very small size, it is, in fact, out a very small portion of a spectacle glass that is employed n any one position of the eye, though its several parts may be ised in succession when any lateral motion is given to the eye. He thence inferred, that the form usually given to such glasses, hough well adapted for other uses in which the rays from all parts of the glass are to be collected into one focus, is not the best for spectacles; but that the best construction would be hat which would give to all parts, separately, the same power of assisting the sight when the eye is turned to each of them n succession. Dr. Wollaston was thus led to the obvious con-clusion, that the form should be (*bombée*) convex without and concave within, so that rays coming to the eye would pass nearly at right angles to the surface of the glass in all directions. These glasses were called by the inventor *periscopic*, and the exclusive sale of them was secured to Messrs. Dollond, by pa-ent.

My attention having been some time since drawn to this sub-ect by an article in Nicholson's Journal, I proposed a trial of hem to M. Cauchoix, well known as a skilful optician in gene-al, and more particularly by the large achromatic lenses which e has lately made of flint glass manufactured in France by M. Dartigues. I requested his opinion on the subject; for, hough our theory should direct the artist, his assistance and xperience are necessary to confirm our results. M. Cauchoix ery soon made several pair of periscopic spectacles of different ocal lengths for the purpose of trying their merits. For, hough Dr. Wollaston had given no measures for the different urvatures of the surfaces, M. Cauchoix, who is conversant ith the theory, as well as with the practice of his art, had no ifficulty in discovering such combinations of curvature as ould answer his purpose. In those which he made first, e exterior surface was nearly concentric with the eye. The upil might then be turned to any extent on each side, and see nearly) as well as through the centre. The field of view ained by this construction is really surprising, and it would equire a person to be for some time trained to the use of the

common

common defective glasses, to be fully sensible of all the superiority of these. For my own part, I have not been accustomed to wear spectacles commonly, and have only used them occasionally for seeing distant objects; but for the last three months I have regularly used the periscopic glasses, and I now shall never employ any others.

There was, however, one inconvenience in those first constructed by M. Cauchoix, which would be felt by those who are in the habit of wearing spectacles constantly. In looking towards a candle, particularly in a theatre where there are many lights, there appeared a variety of reflected images, beside the principal object viewed, which occasioned some confusion. This arose from a combination of reflections between the surfaces, which, in consequence of the degree of difference of their curvatures, occasioned a distinct image to be formed on the retina after two reflections. M. Cauchoix has, however, happily succeeded in removing this inconvenience altogether, by making the inner surface of the glasses less concave* than he did at first, so that whatever light may enter the eye after reflection is no longer brought to a focus, and consequently is not perceived. We have then a larger field than with common spectacles, without introducing any new inconvenience.

During the last three months M. Cauchoix has made trial of these spectacles on a great number of persons, and even upon one so short-sighted, that he could not see beyond the distance of 2¼ inches, which is certainly a case of extreme shortsightedness. All these persons agree in making the same favourable report. The trials made by elderly persons requiring the assistance of convex glasses, have also been attended with just the same success.

I am the more particular in noticing these trials of some months' continuance, because it is by continued trial alone that we can be certain of the goodness of spectacles, and in general of optical instruments that do not magnify much. The eye

* As they have been made from the first by Messrs. Dollond.

has

has a certain flexibility and power to accommodate itself for a short time to a glass that does not quite suit it. But if the same degree of effort is to be long continued, the eye tires and complains of an imperfection that was not at first perceived.

It appeared to me, that so indisputable an improvement upon an instrument generally used, and, indeed, so necessary to many persons, deserved some public notice, and I advise those who ever use spectacles to make trial of these. If they are as well satisfied as I have reason to expect, they will derive a further gratification from reflecting, that the science which thus adds to our enjoyments of the objects immediately around us, is the same that has made us acquainted with the remotest parts of our solar system, and given us some conception of the immense extent of the universe.

(Signed) BIOT,
Member of the Imperial Institute.

---

## IV.

*Observations relative to the near and distant Sight of different Persons. By* James Ware, *Esq. F. R. S. From the Philosophical Transactions for* 1813.

(*Concluded from p.* 288.)

IT appears to militate also against the common observation, that as near-sighted persons grow older, they become less near-sighted; since my eyes, on the contrary, are more near-sighted, at the age of fifty-five, than they were at twenty-five; and I am now obliged to employ deeper concave glasses than I then used to see distant objects, though I am not able to see distinctly through them, things that are near*. Changes in the organ of sight.

The

* It seems difficult to establish any rule (and, perhaps, there may be one) according to which these changes in the eye take place. At

Changes in the organ of sight.

The alteration which has taken place in my range of vision, I have reason to believe, is not unusual. Dr. Wells, in his paper on this subject, mentions the case of a gentleman, who, like me, was near-sighted, and whose sight, as he advanced in life, had undergone a similar change. The following is also an instance of this kind that is still more remarkable. Mr. L., sixty-

fifteen years of age I perceived that many persons saw distant objects better than myself; but the difference being productive of no inconvenience, I did not use concaves till about six years afterwards, and have never used them at all but at the theatre, where I cannot see features without them, or when travelling in the country, or upon similar occasions. The same virtual focus has constantly suited me; and now that my eye has lost its power of adjustment by age, it would be attended with pain to use either a shallower or deeper glass. This focus, ascertained by the distance at which the divergent light from the sun, after passing through the lens, and falling upon paper, forms a circle of twice the diameter of the lens itself, is about three feet; which is the shallowest number of the opticians but one. At all times, before the age of thirty, the habitual distance for reading with the naked eye was six inches, and, when employed upon minute subjects, four. Rather before fifty, it became necessary to use convex glasses, and at present, (sixty) vision at seven inches and a half distant is effected by the assistance of lenses of eleven inches focus. Distant vision by the naked eye requires a distance of two feet. The time at which the power of focal adjustment was lost was not noticed; but from various circumstances this must have happened soon after the age of fifty. No effort can now alter the distinctness of objects either way by the change of which the eye was formerly capable, though spectacles seem less necessary in the morning than later. The power of inclining the optical axes of the eyes appears likewise to have undergone some change: for the separation of objects by squinting is uncomfortable if carried beyond twenty degrees. The act of squinting, however, is in itself uncomfortable with a single object. For if two objects, for example candles, be thus made to afford three images by the coincidence of the two inner images, the eyes will acquire a state of repose, although the axes be much more inclined. In this manner I can produce the coincidence at sixty degrees, whether with distant candles, or by single vision of an object, at the distance of 2·6 inches from the eyes, which is also the distance from pupil to pupil, when the axes are parallel.---W. N.

sixty-six years of age, who has spent a great part of his life in the West Indies, and whose sight, when he was young, enabled him to see both near and distant objects with great precision, began, at the age of forty, to experience a difficulty in reading and writing. He immediately procured convex spectacles of the first number sold by opticians, which glasses are usually ground to a focus of forty-six or forty-eight inches, and by the aid of these he continued to read and write with ease, (distinguishing perfectly in the usual way, all distant objects without them) until he was fifty. At this time he first began to perceive an indistinctness in the appearance of things at a distance; and, on trying with different glasses, he discovered that, by looking through a double concave glass of the sixth number, (which is ground to a radius of eight inches on one side, and eleven inches on the other,) he was enabled to see distant objects distinctly. He has continued to use glasses of this description for the purpose of seeing distant objects from that time to the present; but is obliged to remove them whenever he reads, and still to employ the first number of a convex glass. In this instance a presbyopic was changed to a myopic sight, without any known efficient circumstance to produce it. In the two following cases a similar change took place, and in them it was attributable to known causes. A woman, about fifty years of age, of a full habit, who, for several years had been obliged to make use of convex glasses, in order to read a small print, was seized with a dimness in the sight of the right eye, accompanied with a small degree of inflammation. The sight of the left eye having been long imperfect, this affection of the right eye occasioned a great depression of spirits. Recourse was necessarily had to copious evacuations, by means of which the inflammation and dimness of sight were soon removed; but afterwards the patient was much alarmed on finding that the spectacles she had been accustomed to wear, instead of affording their usual assistance, confused her sight. Upon this discovery, she was induced to look through her husband's glasses, which, in consequence of his being near-sighted, were double concaves of the fifth number, and ground to a radius

Changes in the organ of sight.

Changes in the organ of sight.

radius of eleven inches on each side. These did not assist her in looking at near objects; but by their aid she saw much more distinctly those that were distant; and, on attempting to read, nothing more was now necessary than to bring the book a little nearer to her than she had been previously accustomed to place it. The second case occurred in a patient of the same age, who, in the course of the last year, was attacked with an inflammation in both eyes. By the use of leaches and cooling medicines, it was speedily removed, and afterwards she was much gratified by finding that the necessity for using glasses when she read, which had existed many years, was removed; and that she could see both near and distant objects correctly, without any extraneous help. The amendment in this lady's sight continued, however, only a few weeks; after which she was again obliged to use the same convex glasses in looking at small near objects, which she had used before her eyes became inflamed. In addition to these cases, I beg leave to add the information I have received from an eminent mathematical instrument maker, about fifty years of age, who has long made use of convex glasses to assist his sight in reading. He tells me, that when he has been employed many hours together, for several successive days, in looking through a double microscope that magnifies twenty-eight times, (in order to enable him to mark the degrees on a small brass plate,) he has afterwards been able, repeatedly, for a few weeks, to read without his glasses; but then the amendment gradually ceases, and he is soon obliged to return to the use of the same glasses that he had worn before.

In the instances that have been mentioned, the distant-sightedness affected persons who were considerabty advanced in life: but in the three that follow, a similar affection of the sight occurred in those that were young; and a like good effect was produced by the use of evacuating remedies. One of these was a boy, eight years old, who suddenly became presbyopic, and had repeatedly been punished at school on account of his incorrect and defaced writing: the real cause of it, at that time, being unknown to his master. After the presbyopia had continued

Changes in the organ of sight.

continued a fortnight, and different local applications had been used, without producing any sensibly good effects, the lad was cured by the application of leaches to the temples, and the administration of a few purgative medicines. The other instances occurred in two daughters of the same family. The eldest, twenty years of age, had never been able to do fine work; and, for three years, had been greatly assisted by convex spectacles. The youngest, a girl of fifteen, had become presbyopic about a year ago, and since that time had been obliged to use spectacles whenever she read or worked with her needle. The young person last mentioned, in the course of six weeks (during which time she totally abstained from the use of glasses,) was completely relieved from the necessity of using them by the application of two leaches to each temple twice in a week. The former, in the same space of time, experienced much relief from a similar treatment, but was still unable to do fine work without glasses, partly in consequence of the long continuance of the infirmity, and partly on account of her not having abstained, with equal steadiness, from the occasional use of them.

From the preceding statement the following inferences may be deduced.

First; near-sightedness is rarely observed in infants, or even in children under ten years of age. It affects the higher classes of society more than the lower; and the instances are few, if any, in which, if the use of concave glasses has been adopted, increasing years have either removed or lessened this imperfection.

Secondly; though the usual effect of time on perfect eyes be that of inducing a necessity to make use of concave glasses, in order to see near objects distinctly, yet sometimes, even after the age of fifty, and after convex glasses have been used many years for this purpose, the eyes have not only ceased to derive benefit from them, when looking at near objects, but they have required concave glasses to enable them to distinguish, with precision, objects at a distance.

Thirdly; though the cause of this change be not always known,

known, yet sometimes it has been induced by the use of evacuating remedies, particularly of leaches applied to the temples, and sometimes by looking through a microscope, for a continued length of time in several successive days.

Fourthly; instances are not uncommon, in which persons, far advanced in life, (viz. between eighty and ninety, whose eyes have been accustomed for a long time to the use of deeply convex glasses, when they have read or written, have ceased to derive benefit from these glasses, and they have become able, without any assistance, to see both near and distant objects almost as well as when they were young. Although it be not easy to ascertain the cause of this amended vision, it seems not improbable, that it is occasioned by an absorption of part of the vitreous humour, in consequence of which the sides of the eye collapse, and its axis, from the cornea to the retina, is lengthened, by which alteration the length of this axis is brought into the same proportion to the flattened state of the cornea or crystalline, or both, which it had to these parts before the alteration took place.

---

Additional facts respecting the changes which take place in the eye, by Sir Charles Blagden.

In the same volume of the Transactions, Sir Charles Blagden has given an appendix to this paper. He was not near-sighted at four or five years of age, when he learned to read, but was perceptibly so at nine or ten. He did not become uncomfortably near-sighted till beyond thirty, when the number two or three of the opticians suited his eyes. Since that time, during a few more years, the near-sightedness increased till he was obliged to use No. 5; and at this point his vision has continued stationary between fifteen and twenty years. Sir Charles also mentions an observation on the vertical fluting of a marble chimney-piece, which, in the judgment derived from habit, seemed more remote when the eyes were made to produce a coincidence of different flutings by widening the angle of direction of the optical axes. This circumstance, combined with others, mentioned in Priestley's Optics, and elsewhere,

Other observations on the judgment of distance in near objects from the position of the optical axes.

shews

shews how greatly our inferences respecting the distances of near objects, is governed by a perception of the degree of convergence of those axes. When, at the distance of sixteen feet, I cause two windows which are eight feet asunder, from centre to centre, wholly to coincide by squinting, the eyes become quiescent, and I see three windows, of which the middle one is most distinct, and seems smaller and nearer than the others. And so likewise, when I view the two straight legs of a pair of chemical tongs, held upright at seven inches distance, and produce a coincidence by increasing the convergence of the optical axes, the middle of three legs, which then appear, seems nearer than the others; and if I attempt to touch it by advancing a finger sideways, I find the finger arrives at the point of convergence, or of single vision, at about three inches and a half distance from the eyes. But, on the contrary, when the coincidence is produced by directing the sight to a remote object, the middle leg appears much larger and more distant. This last experiment is less easy to be made than the other, because it is very unusual for us to pay attention to a near object while the organs of perception are adjusted to a remoter one. Among the familiar instances in which the estimate of distance from the inclination of the optical axes comes into operation, may be mentioned the great difficulty of bringing two pens or pencils, or small rods, at once into contact, one being held in each hand, and the ends moved horizontally to each other, while one eye is conversed or kept shut; and the very great advantage to which perspective paintings are viewed by a single eye, or through a tube or instrument which allows one eye only to be used. In either of these cases, the mind can derive no assistance from the position of the optical axes; and in the latter the habitual judgment from the apparent magnitudes, situations, and tints of object, is permitted to give life and reality to the scene, without any controul from that position, which would tend to shew that the whole lay in the same plane.—W. N.

*ERRATA.*

*ERRATA, No. 167.*

The author of the paper on the Wernerian System has sent me the following corrections and addition.

| | | |
|---|---|---|
| Page 150 | line 12 | *dele* the comma after " Pentland-hills." |
| 151 | 4 | *for* " strata disposed," *read* " strata are disposed." |
| 152 | 16-17 | *dele* the comma after " veins," and insert one after " granite." |
| 153 | 12 and marginal note. | *for* " *two* of sandstone," *read* " three," &c. |
| 154 | 15 | *for* newest flœtz-trap," *read* " newest flœtz-trap formation." |
| 159 | 31 | *for* " *alluviæ,*" *read* " *alluvial.*" |

*Note,* to be added at foot of 227, by reference from line 4.

† The propriety of separating the transition rocks from those of the flœtz-class, appears, in fact, to rest on the *practical* necessity of distinguishing formations of such extent and importance from the remaining members of the arrangement. By the remarks in the text, it is not so much intended to deny the convenience of this separation (to judge correctly of which would require more extensive opportunities of observation than the writer has enjoyed) as to shew, that upon this point the arrangement is at variance with the theory, and that, even in a practical view, it may require more distinct elucidation than it has yet received.

# INDEX.

## A.



## B.



## C.

F.



G.



H.



I.







J.



L.



M.

T.



V.



W.



END OF THE THIRTY-SIXTH VOLUME.

G. Sidney, Printer,
Northumberland Street, Strand.

A

# JOURNAL

OF

## NATURAL PHILOSOPHY, CHEMISTRY,

AND

## THE ARTS.

*VOL. XXXVI.*

### ARTICLE I.

*Additional hints and remarks respecting the Equatorial Telescope, and on the visibility of stars in the day-time. In a Letter from Mr.* T. Dick.

*To William Nicholson, Esq.*

SIR,

THE equatorial instrument was contrived, about the middle of the last century, by Mr. T. Short, F. R. S. an ingenious optician, and improver of the reflecting telescope; at least, he is generally allowed to have been the first who adapted a telescope to that machinery. Mr. G. Graham, F. R. S. had indeed previously invented an instrument on a similar principle, which goes by the name of the astronomical or equatorial sector; but its use was limited to finding the difference in right ascension and declination between two objects, the difference of which is

Invention of the equatorial.

is too great to be observed by the micrometer. Mr. Short's instrument seems to have consisted of a complicated apparatus several parts of which are not essentially necessary to an equatorial; and, therefore, must have been very expensive.

Heliostata: supposed to have been a substitute to the equatorial. N. B. It was invented first. See Muschenbroek and S. Gravesande.

Mr. B. Martin in his "Gentleman and Lady's Philosophy," Vol. 2, p. 307, 308, when describing the *heliostata* or planetary clock, a complicated and very expensive piece of machinery, says, that "the equatorial telescope will cost more than double the price of this machine." He alludes to it as consisting of "a large and complicated system of wheel work;" which, however convenient in some respects, is certainly not essentially necessary for producing the requisite movements of the equatorial circles. He also intimates, that the heliostata, which was intended to fix the images of the sun, moon, and planets in astronomical observations, was designed to succeed the equatorial telescope as its substitute. But I have not learned that the heliostata has ever come into general use; for although it accomplishes one object which cannot be effected by the equatorial, viz. in fixing the sun and planets during an observation, yet it cannot be applied to the same general purposes*.

Short's Equatorial.

The different circles in Mr. Short's instrument seem to have been about 6 or 7 inches† in diameter, and were divided by nonius into 3 minutes of a degree, and one minute of time. The telescope adapted to the machinery was a Gregorian reflector,

* Though the heliostata can never supersede the use of the equatorial, it would form an useful appendage to it, or to any telescope mounted on a polar axis; as it must be pleasant, and in many cases useful, to view the heavenly bodies in a quiescent state; particularly, when we wish to delineate a map of the moon, or the spots and phases of the planets. I have not heard, however, of any movement of this kind having been introduced into any of the public observatories in the British empire, except in that belonging to the Trinity college, Dublin, which is furnished with a variety of excellent instruments on a grand scale.

† This estimate is formed from the proportion between the diameters of the circles and the length of the telescope, as represented in the engraving referred to in Martin's Philosophy.

having

having its great speculum 18 inches, focal length ; a kind of telescope by far too unwieldy for circles of the above dimensions. An engraving, with a brief description of Short's equatorial, may be seen in Martin's *System of the Newtonian Philosophy*, vol. 3, p. 399, and in the same vol. p. 89, a representation and description of the heliostata.

The late ingenious Mr. Ramsden, in the construcion of the instrument which he called the *Universal Equatorial*, seems to have considerably simplified Short's equatorial, and at the same time to have rendered it more accurate for astronomical observation. This instrument at once serves all the purposes of an equatorial, and also for observing particular phenomena in the heavens ; having a telescope furnished with magnifying powers extending from 44 to 168. A figure of this instrument may be seen in the Encyclopædia Britannica, vol. 2, Art. Astronomy. Still, however, this excellent instrument was too expensive to be brought into general use ; and to this circumstance, perhaps, is to be attributed the reason, why the equatorial is not as generally known as the theodolite, or the astronomical telescope. Ramsden's Equatorial.

The most simple instrument I have seen, comprising all the apparatus essentially requisite for equatorial movements, is that represented in plate 20 of Adams's Astronomical Essays, 1st edition ; but the substitution of plain sights instead of a telescope, detracts considerably from its accuracy and general use, and prevents its possessor from making many of those observations which can only be made by means of an equatorial. The instrument referred to in the "Celestial Day Observations," published in your Journal for October last, is an improvement on that described by Adams, by the addition of an achromatic telescope, and other adjustments ; and as the circles are divided into 3 minutes of a degree, and 30 seconds of time, it is still more accurate than even Short's equatorial. This instrument, however, in its original state, as it came from the maker's, was not fitted for making observations similar to those recorded in the article referred to ; as the telescope, though it had an Simplest and cheapest Equatorial.

excellent achromatic object glass, was furnished with but a very small degree of magnifying power. My chief design in stating these circumstances, is, with all due deference to their superior skill, to suggest to Messrs. Jones, or any other gentlemen who manufacture such instruments, that, by a slight addition, and variation, they might be made much more interesting to students of astronomy, and capable of more general and extensive application; and their moderate price might induce many who have a taste for astronomical science to procure such an instrument, who, otherwise, might never have thought of purchasing a similar apparatus on a larger scale.

Various improvements proposed to render this instrument more extensively useful.

The improvement to which I allude, is the substitution of a larger telescope, in place of the small one which generally accompanies the instrument, furnished with four eye pieces of different magnifying powers. The achromatic object glass (which should be a very good one) might be from 17 to 22 inches focal distance, and about 2 inches, or nearly so, in diameter. One of the eye-pieces might be a diagonal one, carrying a power of about 20 times; another an erect eye-piece, having a power of 30; and two astronomical eye-pieces, with powers of 60 and 80 times. It would not be necessary that cross-wires should be connected with any more than one of the eye-pieces; as the higher powers would be chiefly used for viewing celestial phenomena by day or by night; in which case the want of the wires is rather an advantage. It will be obvious, too, that in a telescope of such dimensions, furnished with such powers, instead of the sliding tube at the object-end, which produces a tremor in the instrument when adjusting the glasses to distinct vision, the adjustments should be made near the eye-end, by teeth and pinion, as in the larger sort of achromatic telescopes. It will also add to the general utility of the instrument, if one of the eye-pieces be furnished with one of Cavallo's pearl micrometers, which will add but a few shillings to the expence.---The chief objection that can be made to this suggestion will probably be, that the telescope would be too large and unwieldy in proportion to the dimensions of the

equatorial

equatorial circles. This may be obviated by making the tube of the telescope of thin brass, and by placing the object end a little farther from the point of suspension than the eye-end; and by making the horizontal circle a little larger in diameter, and of a very considerable thickness; or by merely extending the screws which support the instrument to a greater distance from each other. But even though these precautions were not taken, no great inconveniency will happen; as the telescope may be set to a very high elevation, without any danger of overturning the instrument. To prevent all possible danger of this kind, the adjusting screws which support the instrument, may be let into a socket, fixed on the pedestal, about half an inch deep, and a slight degree wider than the diameter of the screws, they may also be attached, by means of a silk cord, or a flexible piece of brass, to strong brass pins, fixed near the top of the pedestal.---That all the above suggestions may very easily be brought into effect I have experimental proof.

**Estimate that such an instrument might be afforded for 16 guineas.**

Such an instrument, I presume, could be afforded for 16 guineas. The data on which I proceed in this estimate are these: the instrument alluded to above in its original state, previous to its receiving most of the improvements now suggested, cost 13 guineas: allowing 2 guineas for the value of the small telescope, will reduce the expence of the equatorial circles to 11 guineas; and allowing 5 guineas for the larger telescope now proposed, which is certainly not below a just estimate, the amount will be, as already stated, 16 guineas, which is not much above the price of a $2\frac{1}{2}$ feet achromatic telescope mounted in brass. Nor ought it to form any objection to this proposal, that there would be fewer orders for larger instruments of this kind; as gentlemen of fortune to whom 80 or 100 guineas is no object, and the directors of public observatories, will always prefer instruments of the largest and most accurate construction; and most of those who might purchase such an instrument as the above, would probably have never thought of purchasing any other instrument of the kind. I know several gentlemen who would probably pur-

chase such an instrument, but would not order one without the improvements now described, nor incur the expence of one on a larger scale.

Enumeration of the numerous and extensive advantages of the instrument so improved.

The *advantages* of a small equatorial, with the improvements now suggested, are—It serves as an universal angular instrument for all the purposes of common trigonometrical surveys and measurements.—It forms a *portable observatory* for ascertaining, within a small degree of the truth, the right ascensions, declinations, and other relative positions of the heavenly bodies, and for viewing their various phenomena.—It serves as an universal sun-dial, from which, by a single observation, and without any calculation, the true time may be found to 30 or even 15 seconds: and therefore forms an excellent regulator for adjusting the public clocks of any town or village. By this instrument the position of the meridian may be found from only one observation, and the telescope directed to point at any planet or star, however distant from the meridian, either by day or by night; an object which cannot be effected by any other instrument. It will, of course, serve for detecting the planetary bodies, and most of the stars of the first and second magnitudes, in the day-time. Being furnished with astronomical powers, it serves for viewing the lunar mountains and cavities, the solar spots, the belts and satellites of Jupiter, the phases of Venus, the ring of Saturn, and other celestial phenomena; and therefore supersedes, in a great measure, the use of any other astronomical telescope. It has also this peculiar advantage above a common telescope, that there is no occasion to alter the vertical direction of the telescope, in order to keep the object in the field of view, the apparent diurnal motion of the heavenly bodies being easily followed by the simple application of the hand to a screw, which enables us to view them, for a length of time, with greater steadiness and ease. It is often difficult, when a high power is used, to point a common telescope to an object in the heavens. This difficulty is avoided in the equatorial, by adjusting the telescope to the declination of the body, and when the equatorial motion

is

is performed, the object will immediately appear in the field. When any unusual phenomenon, such as a comet or a new star, appears in the heavens, its true place may, by the machinery of this instrument, be immediately found. Being furnished with a micrometer, it will serve for measuring the apparent diameters of the sun and moon, and the larger planets; and for ascertaining the distance of any terrestrial object, whose dimensions are known. Being furnished with an erect eye-piece, it will serve as a day-telescope for viewing any object on sea or land, which lies within the range of the instrument. In fine, it will serve most, if not all the purposes of the largest equatorials, except in those degrees of precision which are peculiar to instruments constructed on a large scale.

Permanent situation and adjustment.

In order to secure all these advantages, in the use of this instrument, with the least trouble, it will be proper to have it placed on a steady pedestal, of a proper elevation, at a south window, where there is a pretty free horizon. The pedestal should have an iron pin, of about half an inch long, in each foot, to sink into corresponding holes in the floor, to prevent the instrument, after having been adjusted to the meridian, from being shifted from its position. The circle of altitude, too, should generally be kept fixed at an elevation corresponding to the co-latitude of the place, to prevent the trouble of adjusting it at every celestial observation. The equatorial being thus placed and adjusted, will form a pleasant and useful private observatory, and the observations will be made with facility and dispatch.

Advantages of a cheap equatorial.

In conclusion, it may not be improper to remark, that the hints now given are intended merely to recommend the use of a moderate-priced equatorial to those who make the study of astronomy a part of their amusement at leisure hours, and who aim at no more than a near approximation to the truth in the results of their observations, from a conviction, that this excellent instrument is not so much known as its general utility demands. The exquisite precision of modern astronomical observations requires the use of the largest and nicest instruments

ments which can be constructed, and which can be expected to be found only in large public observatories, and in the private ones of gentlemen of fortune. But even with this abatement, the general advantages just now pointed out are sufficient to recommend a small equatorial possessed of the improvements suggested, to every private academy, and to every student and teacher of mathematical and astronomical science.

---

*Query, respecting the visibility of stars in the day-time:*

The visibility of a star in the day-time is as the magnifying power.

It is a fact deduced from every observation I have made on stars in the day-time, and which, I presume, accords with the observations of others, that in proportion as the magnifying power of a telescope is increased, it is rendered more easily distinguishable, and that without a certain degree of magnifying power, a star cannot be rendered visible in day-light.

*Query.* What is the reason that the magnifying power of telescopes produces this effect?

Query. The cause? Not augmentation of sight nor darkness of ground.

In regard to the planets, a probable solution might be given from the consideration that the telescope augments the apparent size of the object, and presents a larger surface to the eye, which is partly the reason why the moon is visible in the day-time. But this reasoning will not apply to the fixed stars, as no telescope is found to augment their apparent size, or make them resemble planetary disks. I am aware, that it may be said, that the telescope excludes almost all the light except what comes from the star; and that by increasing the magnifying power, the ground on which it is seen becomes darker, forming a greater contrast to the light of the star. That these considerations are not sufficient to account for the effect, I am pretty much convinced from the following experiment. I have frequently directed a long tube, blackened on the inside, with a small aperture at the object end, to a star about sunset, when it was easily seen with a very small degree of magnifying power, but could never perceive it through the tube, though I was

was sure it was pointed directly to the star. That the darkness of the ground on which a star is seen, is not, of itself, sufficient to account for the effect produced by magnifying power, appears from the following circumstance, viz. that by diminishing the aperture of the object-glass, we may produce as dark a ground as we please, but this contraction of aperture will not serve to render a star visible, if a small power be applied; nay, the diminution of the aperture beyond a certain limit, prevents a star from being easily seen, which would otherwise be quite perceptible. I am, therefore, induced to conclude, that some additional reasons must be assigned why magnifying power produces this effect. To ascertain these reasons is the object of this query.

Observation. Since each pencil will have the density of its light augmented, at entering the eye, as the square of the magnifying power, so long as the aperture exceeds the pupil in that or a greater ratio—this appears a sufficient reason why a physical point (or small radiant surface) should be more visible.—W. N.

For similar reasons I should wish to be informed if the fact is established *beyond all doubt,* that the stars are visible in the day-time from a deep well or pit, or from the bottom of a high tower. This fact has been asserted by many respectable writers, both ancient and modern, and is so generally taken for granted, that reasons have been assigned to account for the effect; but none of those whose works I have seen, who allude to the fact, ever assert that they themselves, or any of their literary friends, have witnessed this phenomenon. Have any of your numerous scientific readers or correspondents seen the stars in the day-time from a deep pit? Are miners, colliers, and subterraneous surveyors, who have opportunities of making such observations, known to have frequently observed them? If so, are small stars distinguishable in such a situation? Or, is it only when a star of the first magnitude happens to pass near the zenith that such a phenomenon is observed? If the fact is unquestionable, the stars should, in my opinion, for a similar reason, be seen when we look up through a long strait chimney stalk, or through a long tube, where the rays of light are prevented from striking on the inside by the interposition of proper apertures. Is it a fact, as has been asserted, that Tycho Brahe, the Danish astronomer, frequently set in a deep pit and contemplated the stars in the day-time as reflected from a mirror which was placed before him?

Whether stars be really visible by day from deep wells, &c.

An

An answer to the above queries, if they are deemed of sufficient importance for philosophical discussion, will oblige,

Sir,

Your most obedient Servant,

T. DICK.

*Methven, Perthshire,*
*Nov. 16th,* 1813.

---

II.

*A Method of drawing extremely fine Wires.* By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S. From the Philosophical Transactions for* 1813.

Wire of 500 feet to a grain.

IT is recorded by Musschenbroek, that an artist of Augsburgh drew a wire of gold so slender, that five hundred feet of it weighed only one grain; but the method by which this was effected is not mentioned; and indeed it has been doubted whether it could really have been done. I shall, however, shew, that a wire of gold may, without much difficulty, be obtained finer than that spoken of by Musschenbroek, and that wires of platina may be drawn much more slender, with the utmost facility.

Process for drawing wire of ten times that fineness.

Those who draw silver wire in large quantities for lace and embroidery, sometimes begin with a rod that is about three inches in diameter, and ultimately obtain wires that are as small as $\frac{1}{500}$ of an inch in thickness. If, in any stage of this process, a rod of silver wire be taken, and a hole be drilled through it longitudinally, having its diameter one-tenth part of that of the rod; and if a wire of pure gold be inserted, so as to fill the hole, it is evident, that by continuing to draw the rod, the gold within it will be reduced in diameter exactly in the same proportion as the silver; so that if both be thus drawn out together till the diameter of the silver is $\frac{1}{500}$ of an inch, then that of the gold will be only $\frac{1}{5000}$; and of such wire five hundred and fifty feet would be requisite to weigh one grain.

Gold wire.

For

For the purpose of removing the coating of silver that surrounds it, the wire must be steeped for a few minutes in warm nitrous acid, which dissolves the silver without danger of doing any injury to the gold. And though it might be difficult in this manner to preserve any considerable length of such wires, it is of little importance for any of those uses to which it is likely to be applied.

In my endeavours to make slender gold wires by the method above described, the difficulty of drilling the central hole in a metal so tough as fine silver, was greater than I had expected, and I was induced to try whether platina might not be substituted for the gold, as in that case its infusibility would allow me to coat it with silver without the necessity of drilling.

Having formed a cylindrical mould one third of an inch in diameter, I fixed in the centre of it a platina wire previously drawn to the $\frac{1}{100}$ of an inch, and then filled the mould with silver When this rod was drawn to $\frac{1}{30}$, my platina was reduced to $\frac{1}{1000}$, and by successive reduction I obtained wires of $\frac{1}{3000}$ and $\frac{1}{5000}$, each excellent for applying to the eye-pieces of astronomical instruments, and perhaps as fine as can be useful for such purposes*. Very fine wire of platina.

Since this had been the primary object that I had in view, I should have thought my time ill bestowed in pursuing farther the practical application of a method to which there seems no limit, except the imperfections of the metal employed. But as I found by trial the tenacity of these wires to be greater than was to be expected in proportion to their substance, that circumstance excited some doubts regarding the correctness of the estimate by which their diameter had been deduced. Other wires were consequently drawn with the utmost care, as to the quality and substance of the platina employed, and as to the proportional reduction of its diameter in the process of wire-drawing.

* No very accurate observations can be made with a telescope shorter than thirty inches, and at that distance $\frac{1}{4560}$ of an inch subtends only one second of a degree.

The

Experiments chiefly relating to the tenacity and management of such wires.

The extremity of a platina wire having been fused* into a globule nearly one-fourth of an inch in diameter, was next hammered out into a square rod, and then drawn again into a wire $\frac{1}{253}$ of an inch in diameter. One inch of this wire duly coated with silver, was drawn till its length was extended to 182 inches, consequently the proportional diminution of the diameter of the platina will be expressed by the square root of 182, so that its measure had become $\frac{1}{253 \times 13,5} = \frac{1}{3425}$. The specific gravity of the coated wire was assumed to be 10,5, and since the weight of 100 inches was 114 grains, its diameter was inferred to be $\frac{1}{42,8}$ of an inch, or just eighty times that of the platina contained in it.

With portions of the platina wire thus obtained, and successively reduced in diameter, I had an opportunity of repeating the trials of its tenacity with greater confidence in the justness of the estimate, and the results shewed generally (though with some accidental exceptions) that the process of wire-drawing, which is well known to improve the strength of metals within moderate limits, continued also to add something to the tenacity of platina, even as far as $\frac{1}{18,000}$ of an inch, which supported $1\frac{1}{4}$ grain before it broke; but the wire on which these experiments were made, began then to be impaired by repetition of the operation; so that although I afterwards obtained portions of it, as small as $\frac{1}{30,000}$ of an inch in diameter, it was in many places interrupted, and I could place no reliance upon any trials of its tenacity.

Management.

There are some little circumstances in the management of these fine wires, which it may be of advantage to describe for the assistance of those who would apply them to any useful purpose. When the diameter is not less than $\frac{1}{2000}$ or $\frac{1}{3000}$

* I am indebted to my friend Dr. Marcet for the simple and easy method by which the fusion was effected. A piece of wire, about six inches long, having been bent to an angle in the middle, one half of its length was held in the flame of a spirit lamp impelled by a current of oxigen, and its extremity was thus fused in about half a minute.

of

of an inch, the difficulty of seeing and applying them in short pieces is not considerable; but when their diameter is farther reduced, and their length as much as an inch or more, the slightest current of air is sufficient to defeat all attempts to lay hold of an object so difficult to see, and so impossible to feel. It is therefore necessary to retain a part of the silver coating at each extremity, which, at the same time that it assists in finding the end, also serves to stretch the wire with a certain moderate tension, and affords the means of attaching it in any required position.

The method that I have found most convenient, is to bend a portion of the coated wire into the shape of the letter U, with small hooks at its upper extremities. In this form it will conveniently hang upon a wire of gold or of platina, with the lowest part immersed in nitrous acid, till the coating of silver is removed from that part. It may then, without difficulty, be lifted from its place, by one of the hooks alone, to any other situation, or suspended by it, with the other hook downwards, as the means of attaching a small chain, or other series of equal weights in trials of its tenacity.

---

## III.

*An explanatory Statement of the Notions or Principles upon which the systematic Arrrangement is founded, which was adopted as the Basis of an Essay on Chemical Nomenclature. By Professor* Berzelius, &c. &c.

*(Concluded from p. 137.)*

### V. *Oxides of Gold.*

WE come now to such metals as afford no other bases with oxigen, than such as are salifiable, and which by combining with other oxides, seldom contain oxigen in a multiple of Oxides of gold.

of that contained in the oxide combined with them. We shall begin with the oxides of gold.

Former statements of oxides of Gold.

Several chemists have endeavoured to determine the composition of the oxides of gold, but with very different results. M. Proust, for example, found in one experiment, 8·57 per cent. and in another, 31 per cent. of oxigen in this oxide. Richter found that it should contain 20½ per cent. And lately, M. Oberkampf found that the oxide of gold contains 10 per cent. of oxigen. It is known that it is very difficult to produce this oxide in a state of purity, and that part is reduced during the time of washing as well as while drying. In order to obtain a more decisive result than those of my predecessors, I thought it proper to follow a different method.

Auric oxide.

1. Oxidum auricum: I call by this name the ordinary oxide of gold, because I found that gold has also a salifiable oxide, contains less oxigen, and which I therefore call oxidum aurosum.

I dissolved pure gold in the nitro-muriatic acid, and afterwards evaporated the solution to dryness, and until the dried mass began to give out oxymuriatic gas. The neutral muriate thus produced was dissolved in water, and I afterwards digested it with mercury, of which the weight was exactly determined, and did not exceed the half of that of the dissolved gold. I continued the digestion during four days, triturating from time to time the gold which had precipitated. When the gold appeared to contain no more mercury, I decanted the fluid, and washed the gold repeatedly, until the water, by boiling a few minutes, on the metallic powder, did not acquire a yellow colour. I then dried the gold at the temperature of fused lead, and afterwards put it into a small glass retort, in which I exposed it to a red heat, to ascertain with certainty whether the gold contained mercury or not. I then found that a small quantity of mercury had risen in the neck of the retort. This I weighed, and subtracted its weight from that of the mercury employed to precipitate the gold. The experiment was made twice. In the first, 9,335 grammes of gold were precipitated by 14,29

grammes

grammes of mercury; and in the second, 6,557 grains of gold by 9,35 grains of mercury. Oxides of gold.

Now Dr. Seffrom has examined in my laboratory, and while I was present, the exact composition of the oxides of mercury, and has found that 100 parts of mercury combine with 7,9 to 7,99 parts of oxigen, to produce the red oxide. But it is evident that in the foregoing experiments, mercury should form the red oxide, because the quantity of gold dissolved was in such excess, as necessarily to decompose the insoluble muriate of the oxidule, if this were formed. In adopting therefore, the first of these two conclusions of M. Seffrom as the most correct, it follows that in the foregoing experiments, 100 parts of gold gave to the mercury, in the latter 12,003, and in the former 12,077 parts of oxigen; agreeing so nearly together, that I consider these experiments as sufficiently accurate.

It therefore appears that the oxide of gold is composed of

| | | |
|---|---|---|
| Gold........ | 89,225 .......... | 100,000 |
| Oxigen........ | 10,775 .......... | 12,077 |

This analysis is confirmed by the experiments of M. Oberkampf, on the sulphuret of gold obtained by precipitating the muriate of gold with sulphuric gas and hydrogen. He found that 100 parts of gold were combined with 24,39 parts of sulphur, which coincides almost exactly with the calculation, according to the chemical proportions.

Whilst making these experiments, I observed that the oxide of gold has the property of combining with a greater quantity of the muriatic acid, than is required to neutralize it. In which case the muriate, with excess of acid, crystallizes in large prismatic crystals of an orange colour. The crystals are easily soluble in water, and the solution has a bright yellow colour. Exposed to heat, they immediately dissolve in their water of crystallization; and if the heat be continued, the muriatic acid is separated, and the crystals are dried into a mass of a deep ruby colour, which being dissolved in water, gives to it a red colour resembling that of a solution of the red oxide of iron. As it struck me

Oxides of gold. me that the difference between these two muriates of gold might be caused by different degrees of oxidation, I examined the yellow muriate by decomposing it with mercury. It is the latter of the two foregoing analyses, from which it follows that the difference is occasioned by the excess of the acid, and by the water, which are not contained in the red.

2. *Oxidum aurosum.* If the neutral muriate of gold be exposed in a glass vessel to a heat a little above that of boiling water, it begins to give out oximuriatic gas. If left in the same temperature until it no longer continues to discharge the oximuriatic gas, the residue will consist of a saline straw-coloured mass, in which no traces of metallic gold can be discovered by means of the microscope. This salt is not soluble in water; or, in case the red muriate has not been entirely decomposed, the water takes up that portion and becomes of a yellow colour, leaving the new-formed salt undissolved. In this latter case the mass is converted into powder by the action of the water, and the straw-coloured salt forms very small crystalline grains, which do not communicate to water any appearance of dissolved gold. If, on the contrary, the water and the straw-coloured salt be left to macerate, the latter is decomposed and yields a considerable portion of metallic gold, while a red muriate (murias auricus) is dissolved by the water. Boiling water occasions this decomposition almost instantaneously.

The explanation of this phenomenon is very easy. The muriatic acid which is extricated, together with a portion of oxigen before combined with the gold of the muriate, leaves behind a muriate of an inferior degree of oxidation. The same thing happens with the muriate of the oxide of copper, and likewise (as we shall presently see) with the common muriate of platina. The murias aurosus is decomposed by the water, because this having a strong affinity with the murias auricus, it contributes to the formation of this latter, whereby the whole quantity of oxigen combined with the gold in the murias aurosus is concentrated in a part only of the metal. Thus the oxidum

oxidum aurosum is decomposed and converted into metallic gold and oxidum auricum. Oxides of gold.

A portion of murias aurosus, perfectly free from any murias auricus, was decomposed by boiling water. The metallic gold produced was well washed and heated, in order to get rid of any moisture. The solution of the murias auricus, formed by this decomposition, was precipitated with murias ferrosus, and the metallic gold well washed and dried. On examining the weight of these two portions of gold, I found that of the former to be exactly double that of the latter, which result was also confirmed by a careful repetition of the same experiment. It follows, therefore, that the oxidum aurosum contains a quantity of oxigen sufficient to form oxidum auricum with a third part of the gold which it contains. That is to say, the gold in the oxide in *icum*, is combined with three times as much oxigen as it is in the oxide in *osum*. So that the composition of the oxidum auricum being known, that of the oxidum aurosum consequently becomes

| | | |
|---|---|---|
| Gold | 96,13 | 100,000 |
| Oxigen | 3,87 | 4,026 |

Here then we have the first instance of a ratio which rises suddenly from 1 to 3. And this circumstance seems to prove that gold possesses an intermediate degree of oxidation, incapable of becoming saline, because not formed previously to the oxidule.

In order to discover the properties of the oxidum aurosum, I poured on some newly-prepared murias aurosus, a quantity of caustic alkali. The muriate immediately produced a beautiful green colour, and detached a green powder, the lightness of which occasioned it to be suspended in the liquor. When the liquor became clear, it was of a green colour, shewing that the excess of the alkali had dissolved a portion of the oxidule. The oxidule thus formed, was of very short duration. In spite of the pains I had taken to place the phial containing it in a dark room, I found that in half an hour this was covered with a brilliant crust of gold; and after some hours had elapsed,

 the

Oxides of gold. the whole quantity of green oxide was converted into a blackish brown powder, being evidently a mixture of gold and oxidum auricum. The latter of these was separated by means of the muriatic acid which received from it a yellow colour.

The gilded phial, in the foregoing experiment, when held against the light, appeared to be covered with a semi-transparent pellicle of a green colour. In another phial, gilded by the spontaneous decomposition of the oxidum auricum, the colour of the metallic pellicle, when held against the light, was purple ; and in a third phial, where the rays of the sun had dissolved a portion of the muriate of gold, and gilded the surface of the phial, the metallic pellicle was, under the same circumstances, void of colour. The different shades of colour in the metallic pellicle seem therefore to depend on a small portion of the reduced oxide contained in the metal: For if the gilded phial be heated to a red heat, the colour of the pellicle disappears.

By the foregoing experiments, it is proved that the oxidum aurosum may exist without being combined with the acids ; and that in this state it has the form of a powder whose colour is a bright green ; but that it suffers a still quicker decomposition than the oxidum auricum. I have reason, however, to believe, that the oxidum aurosum is, in a small degree, more durable than the foregoing experiment seems to indicate. For in some little time after I had finished this series of experiments, I found that the caustic lixivium I had employed, contained a small quantity of alcohol, by means of which it had been rendered pure. The College of Medicine of Stockholm, ordered me, towards the autumn of 1811, to procure for it the preparations of gold employed by M. Christien for the cure of the venereal disease. One of these preparations was the submuriate of the oxide of gold; which preparation, until M. Oberkampf shewed the contrary, had been considered as a pure oxide. I obtained it by precipitating some neutral muriate with a portion of the forementioned caustic lixivium, which I still had left. Having left the glass containing the mixture during five or six hours in a heated room, I found the precipitation

entirely

entirely effected, forming small crystals of the most beautiful metallic brilliancy, and extremely light. The liquor in which this metallic powder was suspended had much resemblance to the avanturine. Its ætherous smell proved that the reduction of the gold was owing to the presence of a small quantity of alcohol. I have related this experiment, because it furnishes us with the means of reducing gold, with its true colour and metallic brightness, to a state of the greatest possible mechanical division. If mixed in this state with a little of the solution of gum arabic, it may be employed in miniature painting and may be burnished. In the preparations of gold of M. Christien, I employed it instead of the metallic gold reduced according to the prescription from an amalgam of gold by the means of a concave mirror.

Oxides of gold.

N. B.

3. *Gold combined with tin: Purple of Cassius.*

(*a*) A solution of neutral muriat of gold, mixed with a solution of murias stannosus, diluted with a small portion of water, produced a precipitate of a deep brown, almost a black, which had no trace of metallic brightness. After it had been placed on a filtre and well washed, I caused it to be dried. When examined by crushing it on a polished steel plate, it appeared of a metallic brightness, and of a pale yellow colour. On being melted with borax, it produced a metallic button, almost white, or approaching to a yellow. This metallic button, when dissolved in the nitro-muriatic acid, gave a solution of a yellow colour, and containing tin. It also afforded a great quantity of the oxide of tin, not dissolved. The precipitate was therefore a metallic alloy of gold and tin; and in the experiment by which they were produced, the mutual affinity of the gold and the tin had caused a reduction of a portion of the tin, of the murias stannosus, by means of another portion of the same. (*b*) A solution of muriate of gold, mixed with an extremely dilute solution of the murias stannosus, produced, first of all, a red liquid, from which a purple precipitate was slowly deposited. This precipitate, on being washed and dried, was entirely soluble in caustic ammonia, and conse-

 quently

Oxides of gold. quently does not contain any trace of the above-mentioned metallic alloy. An hundred parts of it, when dried and heated to a red heat, in a small glass retort, produced 7·6 parts of pure water without the extrication of any gaziform body. The residue in the retort has now the colour of brick-dust, exactly the same as is obtained by a mixture of the aurum fulminans with sulphate of potash or silica, when the gold has been reduced by heat. Whence it seems to follow, that the powder which remained was nothing but a mixture of metallic gold with the oxide of tin. The brick-dust coloured powder, when treated with the nitro-muriatic acid, gave a solution of gold, from which the murias ferrosus precipitated 25·5 p. c. of metallic gold. The oxide of tin, not dissolved, weighed 66·5 parts. The loss of 0·6 parts must be attributed to the oxide of gold dissolved with the gold.

When I caused the purple not decomposed to be digested with heat, the oxide of tin was dissolved of a yellow colour; and the metallic gold remained. The solution seldom shewed any traces of gold, but it contained both the oxidum stanneum and the oxidum stannicum. These experiments prove, that the purple of Cassius is a triple combination of the oxide of tin (probably of the intermediate oxide) with the oxide of gold and water. The action of heat, together with that of the muriatic acid, produces a reduction of the gold, by means of the intermediate oxide of tin, (oxidum stanneum) which is converted into an oxide *in maximum* (oxidum stannicum*.) The reason why the purple is only formed in very dilute solutions, seems to be, that in a more concentrated solution, the muriatic acid has a stronger tendency to preserve its neutral combination with the oxide of tin. For which reason it is, that only the metallic alloy is precipitated. But when the solution is very dilute, the combination of the two acids

* It is necessary to observe here, that there is, in vol. XXXV, p. 162, an error as to the nomenclature of the oxides of tin. The termination in *eum* should be employed for the intermediate oxide, and the termination in *icum* for the oxide *in maximum*.

acids of gold and of tin is precipitated for the same reason as the oxides of bismuth and of antimony are precipitated by water. Still, however, this operation is not so simple as that the mere action of the water can be supposed to cause the production of the purple. For a mixture of the spirit of Libavius with muriate of gold cannot be precipitated by water; and if caustic alkali be added, this will precipitate a combination of oxide of gold and oxide of tin. This combination is not, however, the purple of Cassius. The precipitate is of a deep brownish blue, and preserves this colour when dried, and even when heated to a red heat. If precipitated with heat, it changes its colour, and becomes of a reddish hue, and contains a mixture of oxide of tin, at the *maximum*, with a metallic alloy of gold and tin. Hence it appears, that the purple is not composed of oxidum auricum and oxidum stanneum, the gold being here combined with a less portion of oxigen than in that oxide. Oxides of gold.

In order to understand the nature of the purple of Cassius, it will be necessary to attend to the circumstances under which it is produced. These are—1. The mutual and very strong affinity existing between gold and tin. 2. The partial reduction of the oxide of the murias auricus, by means of the murias stannosus. And 3. The addition of a quantity of water sufficient to diminish the affinity of the muriatic acid to the oxides with which it is combined. Respecting the first of these circumstances, we have seen that it is on the mutual affinity of their radicals that the affinity of their oxides depends, and that this follows from those electro-chemical principles which were explained at the beginning of this memoir. We have seen, that the black precipitate produced in the murias auricus by the murias stannosus, is a metallic alloy of gold and of tin; and it seems very probable, that this alloy contains those two metals combined in the same proportion as they are in the purple. By way of shewing the strength of their mutual affinity, I shall mention the following experiments. A portion of the black metallic alloy, mixed with sulphur, and melted over the fire, did not suffer any decomposition

Oxides of gold. tion. The metallic button was of the colour of brass, and contained a large portion of tin. Moreover, the purple also, when melted with saltpetre, gives the same metallic alloy, the tin detaching itself from its oxigen and combining with the gold. I dissolved this alloy in some muriatic acid, mixed with a small portion of the nitric. The solution deposited a small quantity of the oxide of tin; and, after having filtered, I evaporated it to dryness, increasing the heat sufficiently to convert the murias auricus into murias aurosus. This I was led to do by the opinion that the tin would evaporate in the form of spirit of Libavius. I was, however, disappointed; for, on pouring water on the mass, which was of a dingy yellow colour, I obtained a solution of murius auricus, mixed with murias stannosus, the remainder consisting of a greenish powder undissolved. I washed this powder, and then macerated it with water. In its decomposition it deposited some metallic gold of a blackish colour, and produced a yellow solution. This solution contained also tin in a large proportion. In these experiments the combination of the two metals was owing to their mutual affinity; (*a*) in the metallic alloy; (*b*) in the purple; (*c*) in the muriate of the oxide; and (*d*) in the muriate of the oxidule; or, in short, in all the forms of combination common to the two metals. It therefore follows, that the mutual affinity of the two metals is the *momentum primum* for the production of the purple.

The *momentum secundum* consists in the partial reduction of the oxidum auricum, which, in this case, should produce an oxide of a degree intermediate between the oxidule and the oxide, where (as we have already seen) a member is wanting in the series. This oxide would be of a purple colour, and would constitute the oxidum auricum. It is evident, that the purple cannot contain any of the oxidum aurosum, because this is of a green colour, both in its solution with the caustic alkali, and in that with the double muriate of oxidum stannosum, and oxidum aurosum, while the purple, in its solution with ammonia, is of a red colour. It also appears, that the purple

Oxides of gold.

purple colour given to animal and vegetable treated with the muriate of gold, is owing to this degree of oxidation. The third circumstance (being the presence of a large quantity of water) tends to weaken the affinity of the muriatic acid. The purple varies in the intensity of its colour according as the liquid is more or less diluted; and with its colour its composition also varies. Not, however, that there exists more than one proportion constituting the true combination; but that, when the quantity of water is too small, the precipitate contains a considerable portion of the black metallic alloy, and when the quantity is too great, a quantity of the white oxide of tin is precipitated with the purple, and gives a greater brightness and transparency to its colour. This will immediately appear on pouring some of the muriate of tin into a large quantity of water, when the oxide of tin will be gradually deposited in the form of a white voluminous, semi-transparent powder.

As to the problem, *how is the purple formed?* its most probable solution seems to be the following. The murias stannosus, diluted with a quantity of water sufficient to diminish the affinity of the muriatic acid to the higher degrees of oxidation of the tin, reduces the murias auricus to the state of an intermediate oxide of a purple colour (oxidum aureum.) A portion of the oxidum stanneum, formed by this reduction of the oxide of gold, combines with the oxidum aureum; and, as the acid can no longer hold them in combination, they are precipitated, whilst another portion of the oxidum stanneum remains in the solution, in form of a supermuriate. If this explanation be the true one, it follows, that the real composition of the purple must take place in such manner as that if the oxigen in the oxidum aureum be = 1, that of the oxidum stanneum will be = 6, and that of the water = 3.

I very much regret, that I have not been able to contrive any experiment whereby this point might be put beyond doubt, and the existence or the non-existence of an intermediate oxide clearly established.

VI.

Oxides of platina.

### VI. The oxides of Platina.

Platina has two oxides, of which however one only has been hither known to us. I took some platina obtained from the triple ammoniacal muriate, and, having dissolved it in the nitromuriatic acid, I evaporated the solution to dryness. I then dissolved this dried mass in water, and again evaporated to dryness, in order to get rid of any excess of acid. The dried muriate was then reduced to a powder, and in the next place exposed to a considerable heat in a sand bath, where I let it remain as long as it continued to give out oxymuriatic gas. It was thus reduced into a pulverized mass of greyish hue, which by candle light seemed rather inclining to a red. This powder was not soluble in water, which, indeed, had scarcely any effect on it. In some experiments the water assumed a yellowish tinge, owing to some of the muriate of the oxide remaining undecomposed. I also observed, that when the muriate of the oxide of platina was obtained by means of common water, containing a small quantity of muriate of natron, this and the oxide of platina remained undecomposed at a temperature which decomposes the murias platinicus. But in this case the double muriate is entirely separated from the murias platinosus by means of water.

The greyish-green powder (or murias platinosus) obtained by this process underwent no immediate change on being exposed to the air. But after the lapse of some months its surface became black. Heated to a red heat it is decomposed, and gives out oxymuriatic gas, leaving a residuum of metallic platina. It is scarcely soluble in the muriatic acid, to which it however imparts a red colour, and from which it may be precipitated by means of an alkali. When macerated for a length of time with the muriatic acid, the murias platinosus is gradually converted into murias platinicus, and is dissolved. At a temperature equal to that of boiling water, the same change is produced by the nitro-muriatic acid. But neither the sulphuric nor the nitric acid occasion in it the slightest change.

*(a.) Oxidum platinosum.* If the murias platineus be digested with a solution of caustic alkali, the muriate becomes black,

black, and the alkali abstracts the muriatic acid from it; the decomposition, however, proceeds but slowly. If the alkali be in excess, this will dissolve a small quantity of the oxidule and assume in the first place a dusky-green colour, changing afterwards to the blackness of ink. On boiling the mixture a part of the oxidule is decomposed, and there are formed metallic platina, a double sub-muriate of the oxide of platina, and kali which is dissolved in the fluid. The oxidum platinicum, when freed as far as possible from the muriatic acid and the alkali, forms a black powder, which suffers no alteration from being dried. If the black ley which is above the oxidule be neutralized by the sulphuric acid, the oxidum platineum is precipitated of a brownish colour, but which appears black when taken on the filtre. If the black oxide be heated till it becomes quite dry, it then gives out water in the first place, and afterwards oxigen gas, leaving as a residuum metallic platina. It is, therefore, an hydrate of the oxidum platineum. By cold digestion with the muriatic acid, the hydrate undergoes no alteration: But if they are boiled together, the hydrate is decomposed and produces murias platinicus and metallic platina. The sulphuric acid does not combine with the oxidum platineum in a dry state; but if the solution of the oxidule in an alkali be precipitated by the sulphuric acid in excess, a blackish solution is then formed, which appears to oxidate by degrees in the air, and then becomes of a more red colour. The concentrated nitric acid dissolves the oxidule not yet dried, the solution being of greenish brown, and when evaporated black. It contains a large portion of the oxide of platina. The concentrated acid also dissolves the oxidule in a moist state. The fluid is of a greenish brown, and affords on evaporation a gummy mass of the same colour, which is soluble in water, but does not deliquesce. The oxidum platineum does not combine with the carbonic acid, for the alkalized sub-carbonates decompose the murias platineus with effervescence. With the muriatic acid and ammonia the oxidum platineum produces a double muriate. I have not been able to produce it by a direct com-

Oxides of platina.

position;

Oxides of platina.

position; but I obtained it by decomposing the double muriate of the oxide platinic and ammonia in a small retort. The double muriate of the oxidule followed the gaziform products, and was condensed in the receiver, mixed with muriate of ammonia, from which I separated it by means of water. It may also be obtained by exposing the double muriate of the platinic oxide to nearly a red heat; for the double muriate of the oxidule is always formed before the platina is reduced. The muriate forms an insoluble powder whose colour is composed of yellow, grey, and green. It suffers no alteration from the concentrated acids; neither does the caustic alkali detach any ammonia from it. But when heated in a retort to a red heat, it is decomposed and yields water, muriatic acid, muriate of ammonia, and metallic platina.

The platineous oxide detonates sharply with burning charcoal; but the muriate of the oxidule remains undecomposed by charcoal only, although it will burn briskly if some sugar or other substance containing hydrogen be added, exactly as we know to be the case with the muriatic acid.

For determining the proportions of the component parts of the platineous oxide I adopted the following method. I made some platineous muriate very dry and then decomposed it by heat in a crucible of platina nicely weighed. I afterwards repeated the same experiment in a retort, in order to ascertain whether the muriate contained any water of combination. But it gave out only oxymuriatic gas. Ten grammes of the platineous muriate left a residuum of 7,33 gr. of metallic platina. But we know that oxymuriatic gas is composed of 100 parts of dry muriatic acid, and of 29,454 parts of oxigen gas.

The muriate consists, therefore, of

$$\text{Oxidule of platina} \begin{cases} \text{radical} & 73{,}300 \\ \text{oxygen} & 6{,}075 \end{cases} = \quad 79{,}375$$

| | |
|---|---|
| Muriatic acid | 20,675 |

Whence it follows that the oxidule should be composed of

| | | |
|---|---|---|
| Platina | 92,35 | 100,000 |
| Oxygen | 7,65 | 8,287 |

*(b.) Oxidum Platinicum.* I digested 20 grammes of mercury,

cury with a neutral solution of the muriate of the oxide of platina, and I changed the solution as often as the intensity of its colour diminished. When a fresh solution, after being boiled during several hours with the metallic precipitate produced by the mercury, suffered no change of colour; I then decanted it and washed well the powdry precipitate, after which I dried it in a heat above that of boiling water. It weighed 10,885 grammes. Exposed to heat in a weighed retort it gave mercury (but which in spite of all possible edulcoration contained traces of the muriate of mercury). It gave no water. The platina had by this means lost 2,334 gr. of its weight, of which I found 2,32 gr. in the fluid mercury. Fearing that the residue in the retort might not be deprived of all its mercury, I put it in a crucible of platina, and heated it by a very strong fire. Its weight was diminished from 8,551 to 8,511, and after that it lost no more weight by successive exposures to heat. It follows then that 2,374 gr. of mercury had contributed nothing to the reduction of the platina; that is to say, that the 8,511 gr. of platina had been reduced by 17,626 gr. of mercury; whence it appears that the platinic oxide must be composed as follows: Oxides of platina.

| | | |
|---|---|---|
| Platina | 85,93 | 100,000 |
| Oxygen | 14,07 | 16,38 |

Thus we see that platina combines with exactly twice as much oxygen in the oxide as in the oxidule, for the small difference of 16,38 and 16,57 can only be attributed to the imperfection of the method of analysis.

It is more difficult than is generally imagined to procure the platinic oxide pure. I have made many experiments thereon, the results of which will be found in my Manual of Chemistry. (Larbok v. Kemien,) p. 422, and p. 429, &c. When we endeavour to precipitate the platinic oxide, we obtain, for the most part, either a double subsalt, or else a combination of the precipitating substance with the platinic oxide.

On examining the precipitate produced by the hydrosulphate of ammonia in a solution of the muriate of platina, I found that this precipitate differs according to the quantity of sulphuret

Oxides of platina.

phuret of hydrogen present. With an excess of the latter, it gives a precipitate of a reddish brown colour, which is decomposed as fast as the sulphuret of hydrogen finds means to escape. The common precipitate appeared to me to be a sulphuret of platina. I have had an opportunity of verifying the observation of Proust that this sulphuret is decomposed, when we attempt to dry it, so that it produces the sulphuric acid, and leaves a sulphuret with a less portion of sulphur. In an experiment where I endeavoured to operate with a sulphuret which had undergone the least possible decomposition by the formation of sulphuric acid, I found that it contained 77 in 100 parts of metal with 23 of sulphur. That is to say, that 100 parts of metal had been combined with 30 parts of sulphur; which proportion is somewhat smaller than it ought to be, according to the calculation of chemical proportions. But the difference must undoubtedly be attributed to a decomposition which had taken place previously to the commencement of the experiment. I found, also, that when the sulphuret was distilled in a retort, a part of its sulphur was disengaged, but another part remained combined with the metal. Hence I conclude, that there are two sulphurets of platina, of which the one is proportional to the oxidule, and the other to the oxide. The sulphuret of platina is dissolved by the hydrosulphuret of kali. The solution is of a reddish brown. The acids precipitate from it a flocculent mass, resembling crocus of antimony. It is the sulphuret of platina, containing sulphuret of hydrogen; but the latter soon flies off when exposed to the air.

*VII. The Oxide and Sulphuret of Palladium.*

Through the complaisance of Dr. Wollaston, I became possessed of a piece of palladium which weighed several grammes. I prepared a gramme in fine filings, which I mixed in a small glass phial with some sulphur. I then heated the mixture over the flame of a spirit lamp, until the metal and the sulphur were combined, which took place, accompanied with the production of fire. I next heated the phial over hot coals, until the excess of the sulphur was entirely got rid of. The sulphurated palladium weighed 1·2815 grains. When mixed

Oxides of palladium.

mixed with another portion of sulphur, and heated again, this sulphuret had received no accession of weight. Whence it follows, that 100 parts of palladium were combined with 28·15 parts of sulphur. The sulphuret of palladium, when exposed to a moderate heat, became covered with a red crust; at the same time discharging sulphureous acid gas. The crust I found to be composed of the sub-sulphate of the oxide of palladium. This subsalt was reduced in a white heat.

I dissolved some palladium in nitro-muriatic acid, which contained hardly any more of the nitric acid than what was necessary to furnish oxigen to the metal. The solution, when evaporated to drynesss, gave a neutral muriate, which I again dissolved in water. I digested, during several days, in this solution two grammes of mercury. It was evident, from the colour, that a considerable portion of the palladium still remained dissolved, and consequently that murias hydrogyrosus (mercurius dulcis) had not been formed. I separated the metallic precipitate from the liquid, and washed it well. It formed a greyish metallic powder, weighing 1·441 gr. This I put into a small retort, and heated it in the flame of a spirit lamp, where I kept it heated to a red heat during half an hour. A small quantity of watery vapour was first of all extricated, and afterwards a little mercury; but which did not continue increasing. Having taken the water from the neck of the retort, I found that this had lost 0·006 grains of its weight, and afterward, when separated from the mercury, that it had lost 0·113 gr. The mercury had therefore weighed 0·112 gr. The metallic powder in the bulb of the retort had suffered no change in its appearance. I put it into a small (creuset de platina), and exposed it to the greatest heat I could possibly produce, keeping it in the fire for half an hour. It left as a residuum a metallic mass of a silvery whiteness, very contracted, and very malleable and flexible. It weighed 0·7073 gr. and lost nothing further of its weight by a subsequent exposure to the fire. The precipitate thus obtained (and weighing 1·441 grains) had therefore been composed of 0·7073 grains of palladium, of 0·112 grains of mercury mechanically adhering,

Oxides of palladium.

adhering, and of 0·6157 gr. of mercury chemically combined with the palladium (that is to say, of 0·7277 gr. of mercury,) and lastly, of 0·006 grains of moisture. It follows, then, that of the two gr. of mercury employed in this experiment, only 1·2725 gr. were oxided; and that these had reduced 0·7073 gr. of palladium. A hundred parts of mercury will, therefore, reduce 55·6 parts of palladium from the state of an oxide, so that the oxide of palladium must be composed of

| | | | |
|---|---|---|---|
| Palladium | 87·56 | - - - | 100·000 |
| Oxigen | 12·44 | - - - | 14·209 |

But, according to the composition of the sulphuret of palladium, 100 parts of palladium should combine with 14·06 parts of oxigen. The experiments do, therefore, in reality, agree, although made on quantities too small to furnish any thing more than approximations.

In making these experiments, I observed that mercury has a very strong affinity to palladium, and that the combination of these two metals may be heated to a red heat without separating the mercury. In order to effect a complete separation of the two metals, it is necessary to keep up a strong fire for a length of time. One circumstance, well worthy of remark, is, that an amalgam of palladium which resisted the action of a cherry red heat was composed of 7073 parts of palladium, with 6157 parts of mercury. Now, the first of these would, for producing oxidation, require 981 parts of oxigen, and the latter 488·8 parts, which wants but little of being exactly the half of the preceding.

The oxide of palladium precipitated with caustic alkali, has the colour of rusty iron. It is an hydrate. This oxide is obtained by exposing the nitrate of palladium to a moderate heat. The nitric acid is detached, and the oxide remains in the form of a black and shining mass. It may be entirely redissolved by the acids, although not without difficulty. It indeed requires continued ebullition to effect it. It is dissolved by the muriatic acid, without the separation of any oxymuriatic gas. The muriate of palladium is of a beautiful red colour, which,

which, however, becomes black if its water of combination be separated from it. If heated in a glass, the muriate immediately melts, and the heat may be raised to a slight degree of redness without decomposing it. By a stronger heat it is decomposed, and the metal is immediately reduced without one's being able to discover any traces of a muriate of oxidule. Hence it appears that palladium has not any oxide analagous to the oxidum platinosum. If the neutral muriate of palladium be dissolved in water, and then evaporated to dryness, it is in part decomposed, the water taking up the muriatic acid; and on dissolving the dried mass, we have for a residuum a rose-coloured mass which is a submuriate of palladium. Oxides of palladium.

### *VIII. The oxides of Manganese.*

In an analysis of cast iron, published in the Afhandlingar i fysik, kemi och Mineralogie, 3 H. p. 141, I have described an experiment made for determining the composition of the oxidum manganicum, in which 100 parts of metallic manganese being dissolved in the pure nitric acid, and the solution evaporated to dryness, and decomposed by a red heat, there was produced 142,16 parts of black oxide. Dr. John obtained, by an experiment analogous with the above, 142 parts of oxide. Dr. John analysed also a solution of the neutral sulphate of manganese, in which there were found, on precipitating with the muriate of barytes, 46,48 parts of oxidum manganosum. It produced 148,5 parts of sulphate of barytes, equivalent to 50,93 parts of sulphuric acid. Now 100 parts of sulphuric acid are neutralised by 91,28 parts of manganeous oxide, and consequently, the 91,28 parts of this oxide should contain 19,96 parts of oxigen. In the manganeous oxide 100 parts of the metal are therefore combined with 28 parts of oxigen. But $42=28\times1\frac{1}{2}$, and the manganic oxide should contain one and a half times as much oxigèn as is contained by the manganeous oxide. Dr. John also found that 100 parts of manganese oxidated by pure water, produced 115 parts of a green oxide, which combined with the liquid acids detaching hydrogen from them, and with the carbonic acid gas reducing

a part

Oxides of manganese.

a part of the same to the state of gaseous oxide de carbon. This green oxide was therefore a suboxide, containing half as much oxigen as the manganeous oxide. For the trifling difference between 14 and 15, may naturally be attributed to some imperfection in the experiment. Bergman observed that manganese kept in a bottle imperfectly closed, falls into a powder; and that this powder has the property of decomposing water. I had an opportunity of verifying this observation of Bergman. Half an ounce of manganese being kept in a bottle closed by a cork, I found, after the expiration of a year, that the metal had fallen into a coarse metallic powder, which, when pounded in a mortar, took a colour that was rather lighter. On being thrown into water, it caused a disengagement of hydrogen gas. In the muriatic acid also, the same effect was produced with great vivacity. As to this powder, is it a suboxide inferior to the green suboxide? I have not yet been able to make any experiment conclusive of this matter. But from those I have made, I shall rather determine it to be a mechanical mixture of carburet of manganese, which does not oxidate in a dry air, and of oxidated manganese.

The *superoxidum manganicum*, (or the native oxide which affords oxigen gas, when heated to a red heat) is not the same as the black oxide of which I have already spoken; for it is reduced by fire to the state of that black oxide (oxidum manganicum). And, as Scheele first observed, the manganeous oxide (obtained by the decomposition of the manganeous carbonate in suitable vessels) has the property of kindling at the temperature of melted sulphur, and then forms the manganic oxide, which is of a blackish brown, or almost as black as the super-oxide. I will not say that the chemists have confounded these two oxides. But, so far as I am informed, they have said nothing positive concerning the marks by which they are distinguished. It is an easy matter, by means of the chemical proportions, to determine what the composition of this super-oxide should be; since it ought to contain twice as much oxigen as the manganeous oxide. In fact, M. Klaproth, in an analysis of a fossil manganic super-oxide found that it yielded

nearly

nearly about ten for an hundred of oxigen gas, which agrees tolerably well with the calculation. Fourcroy maintains that this super-oxide should contain forty for a hundred of oxigen. Metallic oxides, &c.

Adopting the analysis which I made for discovering the composition of the manganic oxide, as the most correct, as it also is the most simple, it follows that the oxides of manganese are composed according to the following statement:

| | metal. | oxigen. | metal. | oxigen. |
|---|---|---|---|---|
| Suboxidum manganicum | 87,68 | 12,32 | 100 | 14,0533 |
| Oxidum manganosum | 78,10 | 21,90 | — | 28,1077 |
| Oxidum manganicum | 70,25 | 29,75 | — | 42,1600 |
| Superoxidum manganicum | 64,00 | 36,00 | — | 56,215 |

## IX. *Experiments on the suboxides of some Metals.*

### (a) *Suboxidum bismuthicum.*

It is well known that bismuth when melted by a moderate heat, becomes covered with a purple powder. The same thing happens if powdered bismuth is exposed for a length of time to the action of the air. I had exposed bismuth in powder to the influence of atmospheric air in an open phial during the summer months. The upper part of the metal I found converted into a powder of a deep purple colour. This part was more divided than the lower part, to which the action of the air had not penetrated. There was a very marked limit between the metallic part and the part suboxided. I took away some of this latter, which I treated with the concentrated muriatic acid. The suboxide was decomposed, and the result consisted of muriat of bismuth, and bismuth reduced.

I must here remind the reader, that it is one of the characters of this class of oxided bodies, not to combine with other oxides, without undergoing a similar decomposition.

### (b) *Suboxidum plumbicum.*

We know that lead when exposed to the atmosphere, becomes covered by degrees with a pellicle of a deeper colour than

Metallic oxides, &c.

than that of the metal which thus loses its metallic brightness. This pellicle is more readily formed, if the lead be heated to nearly a melting state. The surface of the metal is tarnished, and becomes of a deeper and deeper colour, till it at length appears almost black. But if the heat be increased so as to melt the lead, the pellicle in that case will break, and in a few moments we shall find it converted into a yellow oxide of lead. The same result is produced, if one half of the surface of a piece of lead be covered with a transparent varnish, leaving the other half uncovered. This latter will change its colour by degrees; and if, after some time, the varnish be removed, we may then observe the different appearance of the metallic and the suboxided surface.

I had some hopes of obtaining this suboxide by means of the action of the electric pile, and in a quantity sufficient to enable me to analyse it. For this purpose I employed wires of lead as conductors in pure water. The decomposition of the water took place slowly. No change was perceptible in the positive wire, but the negative wire was by degrees covered with a vegetation of metallic lead. The cause of its production will readily appear on recollecting the observation of M. Gayton Morveau, that the oxide of lead is soluble in pure water.

I then thought of obtaining this suboxide by the following method: I combined some lead with mercury, and shaking the amalgam in a bottle, I took away from time to time the black powder which was thus formed. I considered this powder as the suboxide sought for; but when I triturated it in a mortar, I saw that it was almost entirely reduced to a fluid amalgam. A very small part, however, remained in the form of a powder; but this powder, when rubbed against gold, amalgamated its surface. This experiment proves that the black powder was really nothing more than metallic molecules, whose reunion was prevented by a pellicle of suboxide infinitely small, and which was broken by the friction. If those oxides of lead of which we know the composition be exact

multiples

multiples by 2, 3, and 4, it follows that this suboxide should be the *minimum*, and should contain 3,65 parts oxigen for 100 of the metal. Metallic oxides, &c.

(*c*) *Suboxidum Zincicum.*

Zinc becomes covered in the air with a pellicle which is of a grey colour, hard, semimetallic, and of difficult solution with the acids. Its exterior considerably resembles the oxides of kalium and of natrium. This suboxide is in other respects already too well known through the experiments with the electric pile, in which it has occasioned a good deal of difficulty in the cleaning the plates of zinc. I therefore deem it needless to instance any experiments by way of proving its existence.

The existence of the suboxides of kalium and nutrium, having been proved by the experiments of Mr. Davy, Gay Lussac and Thenard, and it being also shewn, by the experiments which I have related, that only antimony, manganese, bismuth, lead, and zinc, have suboxides.

It is very probable that we shall in time discover also the suboxides of most of the metals. There is, however, one circumstance which I must not allow to pass unnoticed, and which, in the opinion of many of my readers, may throw a doubt over what I have just been saying; namely, that not only Mr. Davy, but Messrs. Gay Lussac and Thenard, have in their latest writings endeavoured to prove, that the suboxides of kalium and natrium, ought rather to be considered as mixtures of those alkalies, each with its metallic radical. But as I have myself obtained these suboxides in a most decided form, I must, notwithstanding the opinion of these celebrated chemists, still maintain, that such a mixture of the alkali, with its radical, cannot be the same as with the suboxide.

I am inclined to believe that copper, gold, platina, and mercury, have no suboxides, and that their *oxida in ostum*, have, in their composition, a proportion relative to that of the above-mentioned suboxides. For these oxidules, if treated in their

dry state, with the acids, are for the most part decomposed into *oxida in icum*, and reduced metal.

---

## IV.

*Description of a single-lens Micrometer.* By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S. Phil. Trans.* 1813.

A single lens is employed to magnify;

HAVING had occasion to measure some very small wires with a greater degree of accuracy than I was enabled to do by any instrument hitherto made use of for such purposes, I was led to contrive other means that might more effectually answer the end proposed. The instrument to which I had recourse, is furnished with a single lens of about one-twelfth of an inch focal length. The aperture of such a lens is necessarily small, so that when it is mounted in a plate of brass, a small perforation can be made by the side of it in the brass, as near to its centre as one-twenty-fifth of an inch.

and the object of its natural size is seen by the same eye through a perforation.

When a lens thus mounted is placed before the eye for the purpose of examining any small object, the pupil is of sufficient magnitude for seeing distant objects at the same time through the adjacent perforation, so that the apparent dimensions of the magnified image might be compared with a scale of inches, feet, or yards, according to the distance at which it might be convenient to place it. A scale of smaller dimensions attached to the instrument, will, however, be found preferable on account of the steadiness with which the comparison may be made; and it may be seen with sufficient distinctness by the naked eye, without any effort of nice adaptation, by reason of the smallness of the hole through which it is viewed.

The construction that I have chosen for the scale, is represented in fig. 1. (See Plate VI.) It is composed of small wires, about 1-50th of an inch in diameter, placed side by side, so as to form a scale of equal parts, which may with ease be counted by means of a certain regular variation of the lengths of the wires.

*Description of the instrument.*

The external appearance of the whole instrument is that of a common telescope, consisting of three tubes. The scale occupies the place of the object glass, and the little lens is situated at the smaller end, with a pair of plain glasses sliding before it, between which the subject of examination is to be included. This part of the apparatus is shewn separately in fig. 3. It has a projection at *a*, with a perforation through which a pin is inserted, to connect it with a screw represented at *b*, fig. 2. This screw gives lateral motion to the object, so as to make it correspond with any particular part of the scale. The lens has also a small motion of adjustment by means of the cap. *c*, fig. 2, which renders the view of the magnified object distinct.

Before the instrument is completed, it is necessary to determine with precision the indications of the scale, which must be different according to the distance to which the tube is drawn out. In my instrument, one division of the scale corresponds to $\frac{1}{10000}$ of an inch, when it is at the distance of 16,6 inches from the lens; and since the apparent magnitude in small angles varies in the simple inverse ratio of this distance, each division of the same scale will correspond to $\frac{1}{5000}$ at the distance of 8 $\frac{3}{10}$ inches, and the intermediate fractions $\frac{1}{6000}$, $\frac{1}{7000}$ &c. are found by intervals of 1,66 inch marked on the outside of the tube. The basis on which these indications were founded in this instrument, was a wire carefully ascertained to be $\frac{1}{200}$ of an inch in diameter, the magnified image of which occupied fifty divisions of the scale, when it was at the distance of 16,6 inches, and hence one division $= \frac{1}{50 \times 200} = \frac{1}{10000}$. Since any error in the original estimate of this wire must pervade all subsequent measures derived from it, the substance employed was pure gold drawn till fifty-two inches in length, weighed exactly five grains. If we assume the specific gravity of gold to be 19,36, a cylindrical inch will weigh 3837 grains, and we may thence infer the diameter of such a wire to be $\frac{1}{200}$ of an inch, more nearly than can be ascertained by any other method. For the sake of rendering the

Measurement of a very minute object or angle.

the scale more accurate, a similar method was in fact pursued with several gold wires, of different sizes, weighed with equal care; and the subdivisions of the exterior scale were made to correspond with the average of their indications.

In making use of this micrometer for taking the measure of any object, it would be sufficient at any one accidental position of the tube to note the number on the outside as denominator, and to observe the number of divisions and decimal parts, which the subject of examination occupies on the interior scale, as numerator of a fraction expressing its dimensions in proportional parts of an inch; but it is preferable to obtain an integer as numerator, by sliding the tube inward or outward, till the image of the wire is seen to correspond with some exact number of divisions, not only for the sake of greater simplicity in the arithmetical computation, but because we can by the eye judge more correctly of actual coincidence, than of the comparative magnitudes of adjacent intervals.

The smallest quantity, which the graduations of this instrument profess to measure, is less than the eye can really appreciate in sliding the tube inward or outward. If, for instance, the object measured be really $\frac{1}{9900}$, it may appear $\frac{1}{10000}$ or $\frac{1}{9800}$, in which case the doubt amounts to 1-50th part of the whole quantity. But the difference is here exceedingly small in comparison to the extreme division of other instruments where the nominal extent of its power is the same. A micrometer with a divided eye-glass, may profess to measure as far as $\frac{1}{10000}$ of an inch; but the next division is $\frac{2}{10000}$ or $\frac{1}{5000}$; and though the eye may be able to distinguish that the truth lies between the two, it receives no assistance within $\frac{1}{2}$ part of the larger measure.

On

V.

*On the Electric Column. By* George John Singer, *Lecturer on Experimental Philosophy, &c. (From the Author.)*

SOON after the invention of the electric column, I instituted a series of experiments on its construction and properties, which have been continued, at every interval of leisure, to the present time.

Explanation of the difference between the Electric Column, and the Pile or Voltaic apparatus.

The distinction between that interesting instrument, and the voltaic apparatus, always appeared to me to arise from the nature of the substance interposed between the different pairs of plates—a *liquid*, whose parts have freedom of motion, being essential to the proper action of the voltaic apparatus, whilst that condition is unnecessary in the electric column, which requires only the combination of its pairs of metal plates by some conductor that does not counteract their electromotive power.

The discs of paper usually employed for this purpose owe their conducting faculty entirely to the water they contain, perfectly dry paper being a non-conductor. But this perfect dryness can only be obtained by exposing the papers to a heat nearly sufficient to scorch them, as those who endeavour to excite that substance by friction, will soon acknowledge. And the attraction of paper for that small quantity of water, which renders it conducting, is such, that in the ordinary state of the atmosphere it resumes its original conducting power in a few minutes after it is removed from the fire.

The permanent action of the electric column appears to depend on the condition that there be no sensible change in the nature of the substances of which it is composed, and therefore, within certain limits, the drier the pieces of paper, the more completely will this object be accomplished, provided they still retain sufficient conducting power. I have found that they do so even when the column is put together under the full influence of the sun's rays, and at a time when the heat they produce is sufficient to warp the paper discs considerably;

Observations on the effect of heat upon the electromotive power of the column.

derably; and I have also found that by increasing the natural moisture of the paper artificially, the effects of the columns are rendered less permanent.

The peculiar object of my experiments has hitherto prevented me from attending very accurately to the meteorological relations of this instrument, though I have contrived some simple arrangements of the apparatus for that purpose; but in the course of these inquiries I have particularly remarked, that heat has a very manifest influence on the electrical power of the column, and I am induced to ascribe to that cause, the effect produced by the action of the sun's rays, as described by its ingenious inventor, Mr. De Luc, in the last number of this Journal.

That active philosopher is at present of a contrary opinion, to which he has been led by considering the effect produced by heat when its action is so considerable as to dry the paper discs, and render them non-conducting. But he is not yet acquainted with the facts I have now to detail.

1. I have a small bell-ringing apparatus, which is constantly in motion, but the pendulum vibrates with different velocity at various times. During a long series of observations, it has been found, that the motion is always slower in winter than during the summer, if the apparatus is kept in a room where there is no fire; and that any considerable increase of temperature is soon followed by an increased velocity of motion.

2. A bell-ringing apparatus, which had been very active during the summer, began to pulsate more slowly towards the middle of September: the apparatus was then removed into another room, and placed at some distance from the fire. At the end of a quarter of an hour it vibrated more rapidly than at any former period; but on being removed to its former situation, the action gradually declined, and became as slow as before.

3. A column of 1000 series was applied to the cap of a gold leaf electrometer, its remote extremity being held in the hand. The temperature of the room was about 50. The gold leaf struck the sides of the glass nine times in 64 seconds. The

The column was then placed before a fire, in a situation where the thermometer rose to 80. In ten minutes it was removed, and applied to the electrometer; the gold leaf struck the sides of the glass 37 times in 60 seconds. The column was returned to its former situation until cooled to the original temperature: it then produced nine strikings in 60 seconds. It was placed again before the fire for ten minutes, and, on its application to the electrometer, produced 36 strikings in 60 seconds nearly.

Observations on the effect of heat upon the electromotive power of the column.

Some effects, nearly analogous to these, were observed with the usual voltaic apparatus, by M. Desseignes His experiments are detailed in the Journal de Physique, vol. 73, p. 73 and 417. I shall probably refer to them in a future paper.

I think it necessary to mention here a caution essential to the uniform action of the electric column. That instrument should never be left for any considerable time with its opposite ends connected by a conducting substance, as is the case when it is simply laid on a shelf or table, or in a drawer; for, under such circumstances, it will appear to have very little power when first applied to the cap of an electrometer, and will require some time to recover its original intensity. The best arrangement, when the column is laid by, is to place it resting on two sticks of sealing wax, so that its brass caps may be kept at the distance of half an inch from the table, or other conducting surface on which it is disposed.

*Princes Street, Cavendish Square,*
*Dec. 13th*, 1813.

VI.

*Chemical Examination of a supposed variety of Apatite or Native Phosphate of Lime, from Aberdeenshire, in Scotland. By* AMERICO CABRALL DE MELLO. *(From the Author.)*

On the classification of mineral bodies.

AS the philosophy of the subjects of the mineral kingdom must naturally be in a constant state of improvement, every attempt to extend its boundaries by new researches, cannot fail to prove useful to the progress of science.

And although the science of minerals has of late been prodigiously extended, our knowledge with regard to the chemical constitution of a vast number of the inorganic bodies which compose the shell of our earth, is still confined within narrow limits; not only are we ignorant of the composition of a multitude of the most common fossils we tread under our feet, but even with respect to those with which men appear to be acquainted, further information is required.

The constitution of mineral substances being so widely different from that of organic beings, gives rise to such infinite varieties of forms and arrangements of principles, as will probably for ever prevent the mineralogist to establish his system upon constant and invariable characters. The formation of fossils being the result of simple combination of external materials, aided by mechanical action, or effected by chemical affinity, must produce varieties of forms, which can only be arranged by arbitrary titles.

It is thus that we see minerals so frequently removed from one class into another. Indeed, this methodical distribution of the subjects of natural history, whatever steps be taken, must, on this account, be constantly changeable, and cannot be so absolute as those which belong to animals and vegetables.

The mineral of which I herewith subjoin the analysis, having been considered as a new variety of phosphate of lime, it has received a place as such among the native phosphates in the cabinets of mineralogists of this country. It was brought into

into notice by Mr. Sowerby, who has given of it a description and drawing, in his British Mineralogy. Analysis of a fossil.

*Natural History of the Fossil.*

It occurs massive, accompanied by granit, in the hill called *Tyre-Beggar*, near Dyce, in Aberdeenshire. The colour of this fossil is a pale greenish white. It becomes phosphorescent, and shines with a greenish light when heated to a dull redness, for *a few minutes only*. A more intense and longer continued heat, destroys its capability of shining in the dark. On exposure to moist air, it slowly disintegrates and becomes covered with a white dusty efflorescence. Its recent fracture exhibits a silky lustre, and appears to be composed of bundles of very minute needle-shaped crystals, scarcely distinguishable by the naked eye. It is very hard, but may be scratched with a steel point. It is copiously interspersed with dodecahedral garnets of the size of large pease. Its specific gravity is 2·95.

*Analysis.*

In order to examine the agency of acids on this mineral, I introduced detached portions of it, finely pulverised into four separate flasks, and poured upon one, sulphuric acid, upon the other, nitric acid, and upon the third, muriatic acid moderately diluted, and subjected them for some hours to a gentle heat.

From this experiment I learnt that the nitric and sulphuric acids had produced no change, because the mineral was again obtained from the acids unaltered in weight, and qualities, by mere ablution with water. The muriatic acid alone produced with the fossil, by long-continued digestion, a gelatinous fluid. 150 grains of it, exposed to a red heat, lost 3 grains. It did not fuse.

Learning from these preliminary steps, that the analysis could not be effected by the agencies of acids, I heated the mineral with charcoal powder to redness. This mass, when moistened with water, evolved a strong odour of sulphuretted hydrogen, a proof that sulphuric acid was present in the fossil.

A. 150

Analysis of a fossil.

Process A. 150 grains of the pulverised stone were therefore boiled for some hours with 450 of subcarbonate of potash, in about 2000 of water. The mixture was filtred, and the residue washed and dried.

B. The residue of the foregoing process, was transferred into dilute nitric acid, which effected a partial solution, accompanied with a strong effervescence. When no further action ensued, the liquid was decanted, and the remainder washed with water.

C. The insoluble part (B) being in a similar manner treated alternately, first with subcarbonate of potash, and then with nitric acid, until the acid when poured on the powder, which had been boiled for some hours in the alkaline solution, produced no further action, the insoluble residue weighed 25,50 grains. It resisted muriatic and nitric acid, but fused with soda, before the blowpipe apex, into a yellowish glass. It therefore was silex.

D. To ascertain whether phosphoric acid was contained in the nitric solution, a portion of it was assayed with nitrate of lead, &c. but no vestige of this acid was discovered.

E. All the before obtained nitric solutions, after being mingled, were decomposed by the admixture of subcarbonate of potash; a precipitate was obtained, and this being dried, was covered with concentrated sulphuric acid, and heated in a platina bason, to a dull redness.

F. The dry mass produced, after being levigated and digested in small portions of cold water, was thrown on a filtre, and this residue, or sulphate of lime, dried at a red heat. It acquired, during this process, an orange red colour.

G. The liquid obtained in process F, was decomposed by subcarbonate of potash, and yielded a white powder, which did not dissolve in potash. It weighed 4 grains after having been kept in a red head for about one hour. It was magnesia.

H. The sulphate of lime obtained in process F, was digested repeatedly in muriatic acid, which deprived it of colour, and rendered it perfectly white, after being washed with water.

I. The muriatic solution, from which the sulphate of lime had

had been separated, being mingled with liquid ammonia, yielded 3·50 gr. of oxide of iron, and this being deducted from the product, obtained by process H, left the quantity of sulphate of lime = 114 gr. Analysis of a fossil.

The examination being thus completed, it is obvious that the mineral cannot be classed among the *apatites*, or native phosphates of lime; but that it is a variety of sulphate of lime, the chemical constitution of which is as follows: viz. 150 parts contain

| | |
|---|---|
| Sulphate of lime............ | 114 |
| Silex...................... | 25·50 |
| Oxide of iron............... | 3·50 |
| Magnesia................... | 4 |
| Water...................... | 3 |
| | 150·00 |

*5, Nassau Street, Soho,*
*Dec* 9, 1813.

# VII.

# METEOROLOGICAL JOURNAL.

| 1813. | Wind. | Barometer. Max. | Min. | Med. | Thermometer. Max. | Min. | Med. | Evap. | Ra | |
|---|---|---|---|---|---|---|---|---|---|---|
| 10th Mo. | | | | | | | | | | |
| Oct. 16 | W | 29·20 | 28·74 | 28·970 | 51 | 38 | 44·5 | — | ·19 | ☽ |
| 17 | S W | 29·01 | 28·64 | 28·825 | 52 | 42 | 47·0 | — | 8 | |
| 18 | N W | 29·55 | 29·44 | 29·495 | 58 | 35 | 46·5 | — | | |
| 19 | Var. | 29·66 | 29·55 | 29·605 | 50 | 27 | 38·5 | — | | |
| 20 | E | 29·66 | 29·50 | 29·580 | 52 | 42 | 47·0 | ·12 | ·14 | |
| 21 | Var. | 29·76 | 29·50 | 29·630 | 54 | 43 | 48·5 | — | — | |
| 22 | N E | 29·89 | 29·45 | 29·670 | 53 | 40 | 46·5 | — | ·10 | |
| 23 | E | 29·91 | 29·86 | 29·885 | 52 | 51 | 51·5 | ·12 | | |
| 24 | N E | 29·86 | 29·82 | 29·840 | 57 | 45 | 51·0 | — | | ● |
| 25 | N E | 30·12 | 29·82 | 29·970 | 53 | 37 | 45·0 | — | — | |
| 26 | N E | 30·12 | 29·85 | 29·985 | 47 | 36 | 41·5 | — | | |
| 27 | N E | 29·79 | 29·77 | 29·780 | 41 | 34 | 37·5 | ·17 | 6 | |
| 28 | N | 29·90 | 29·79 | 29·845 | 46 | 33 | 39·5 | — | | |
| 29 | E | 29·90 | 29·70 | 29·800 | 47 | 27 | 37·0 | — | — | ☾ |
| 30 | Var. | 29·70 | 29·00 | 29·350 | 52 | 30 | 41·0 | — | ·50 | |
| 31 | W | 29·25 | 29·00 | 29·125 | 56 | 30 | 43·0 | — | — | |
| 11th Mo. | | | | | | | | | | |
| Nov. 1 | W | 29·68 | 29·25 | 29·465 | 49 | 31 | 40·0 | — | — | |
| 2 | N W | 29·83 | 29·41 | 29·620 | 49 | 35 | 42·0 | 8 | 9 | |
| 3 | S W | 30·32 | 29·83 | 30·075 | 51 | 29 | 40·0 | — | — | |
| 4 | W | 30·34 | 30·32 | 30·330 | 45 | 27 | 36·0 | — | | |
| 5 | S | 30·32 | 29·95 | 30.135 | 47 | 29 | 38·0 | — | | |
| 6 | E | 29·95 | 29·70 | 29·825 | 45 | 35 | 40·0 | ·14 | 6 | |
| 7 | S W | 29·70 | 29·37 | 29·535 | 53 | 41 | 47·0 | — | ·17 | |
| 8 | S W | 29·42 | 29·37 | 29.395 | 58 | 43 | 50·5 | — | ·22 | ○ |
| 9 | W | 29·62 | 29·42 | 29·520 | 51 | 42 | 46·5 | — | — | |
| 10 | W | 29·60 | 29·48 | 29·540 | 57 | 43 | 50·0 | — | ·10 | |
| 11 | S W | 29·65 | 29·51 | 29·580 | 54 | 44 | 49·0 | ·17 | — | |
| 12 | W | 29·49 | 29·45 | 29·470 | 54 | 32 | 43·0 | — | ·35 | |
| 13 | S W | 29·58 | 29·49 | 29·535 | 46 | 29 | 37·5 | — | | |
| 14 | W | 29·58 | 29·17 | 29·375 | 43 | 33 | 38·0 | 3 | 8 | |
| | | 30·34 | 28·74 | 29·625 | 58 | 27 | 43·41 | 0·83 | 2·14 | |

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## REMARKS.

*Tenth Month.* 16. Fine morning; wet p. m. lightning in the evening. 17. Showery. 18. Fine a. m. shower p. m. 19. Hoar frost; fair day after misty morning. 20. Cloudy a. m. much wind at E. 22. About half past 7, p. m. a bright blue meteor appeared in the N. and, passing to westward with a steady and rather slow motion, became extinct. There were traces of cirrostratus clouds, which increased afterward. 23. Maximum of temperature at 9 a. m. Cloudy, with a breeze. 24. Overcast. 25. Cloudy a. m. clear p. m. During the twilight there was an abundance of red haze, first in the E. horizon, over clouds in that quarter, then at a considerable elevation in the W. It ended more clear and orange-coloured. 26. Cloudy at intervals. 27. a. m. windy. 28. Hoar frost, which being examined, was found to consist, not of spiculæ attached to the herbage, but of the drops of dew, frozen clear and solid. 29. Hoar frost; cumulostrati followed by nimbi. One of the latter approaching from the E. at 4 p. m. exhibited a double rainbow, on a ground of purple. 30. Spicular hoar frost; very misty; clear at noon: cirrostratus and a little rain, p. m. wet and stormy night. 31. Windy a. m. cirrus with cumulus: a shower about 4, with a fine bow.

*Eleventh Month.* 1. Hoar frost. 2. Granular hoar frost: very clear sun-rise: clouds at noon: rain p. m. very windy night. 3. Sunshine, a. m. wind N. N. W. a shower p. m. 6. Calm clear weather, with hoar frost, these three days, the wind now rising. 7. Small rain at intervals: a *solar halo* of large diameter, p. m. 8. Fair a. m. but before 4, dark nimbi and rain beginning. Being on the south side of London, I was surprised with a flash of lightning and a sharp peal of thunder: about half past 6, a glimpse of a meteor passing to the W. It was said to lighten after this time. 8. 9. Windy. 11. Stormy night. 12. Windy; cloudy p. m. wet. 13. Hoar frost. 14. The same, with a crystallized rime on the shrubs.

## RESULTS.

Winds variable: the easterly prevailed in the former, the westerly in the latter part.

Barometer: greatest height 30·34 in.: least 28·74 in.
Mean of the period 29·625 inches.

Thermometer: greatest height 58°: least 27°;
Mean of the period, 45·41°.

Evaporation, 0·78 in. Rain 2·14 in.

L. HOWARD.

TOTTENHAM.
*Eleventh Month, 17th*, 1818.

# SCIENTIFIC NEWS.

## *Geological Society.*

*Dec. 3d,* 1813.

The President in the chair.

The Right Honourable the Earl of Hardwicke,
George Croker Fox, Esq. of Falmouth,
William Stewart Rose, Esq. Palace Yard,
Thomas P. Smith, Esq. of Stoke Newington,
were severally elected members of the Society.

A paper, intitled "Memoranda relative to the Porphyritic Veins of St. Agnes, in Cornwall," by the Rev. J. J. Conybeare, M. G. S. was read.

The veins described in this paper occur on the coast between St. Agnes and Cligga point, traversing or lying on the surface of rocks of tortuous killas. The veins themselves vary in thickness from forty feet to half an inch. Their general character is porphyritic consisting of a base composed of minutely aggregated quartz, mica, talcite, and probably felspar, in which are imbedded grains and crystals of quartz, feldspar, chlorite, mica, and talcite, in small patches. Sometimes the porphyritic character is superseded by a more completely crystalline one, approaching to granite, and containing small veins of tin stone. Sometimes again the veins consist of quartz and tourmaline, forming a rock very nearly resembling that of St. Roche.

The killas adjacent to the veins is more crystalline than elsewhere, and sometimes is scarcely to be distinguished from gneiss. Mr. Conybeare considers the veins and the rock in which they occur, to be of contemporaneous origin.

A paper intitled "a Description of some specimens from the neighbourhood of Cambridge," by Henry Warburton, Esq. M. G. S. was read.

These

These specimens formed part of a bed of rubble, covering the summit of a hillock of grey, or the lower chalk, about five miles S. W. of Cambridge. This hillock, like several others in the same county, is situated to the west of the great range of chalk, being surrounded by the blue marl or gault, as it is provincially termed, from which the overlying bed of chalk is separated by a thin bed of green sand. The rubble, besides, consisting of chalk and flint, also contains shell limestone, angular pieces of green stone, and certain organic remains belonging to older beds than the chalk: but as all these beds basset more or less to the west of the place where these fragments are now to be found, the circumstance is considered by Mr. Warburton as indicating an ancient current, the course of which was from west to east.

A paper entitled, "Observations on Glen Tilt," by Dr. Mac Culloch, V. P. G. S. was also read.

That part of Glen Tilt, which is the subject of the present paper, extends four or five miles from Forest Lodge to Gow's Bridge. It consists of primitive schist, assuming the appearance of clay, of mica slate, and of horneblende slate, with which are interstratified various beds of granular limestone more or less micaceous. Near Gow's bridge the stratification is perfectly regular and uninterrupted, but higher up towards the Lodge, it is traversed by granite rock, and an infinite multitude of granite veins of various sizes. Where this latter rock makes its appearance, the even course of the schistus is interrupted in proportion to the magnitude of the mass of granite. When the granite, schist, and limestone are all in contact, a perfect confusion of these three substances takes place. Where the granite and limestone are in contact, the latter is highly indurated, and penetrated by siliceous matter.

*Dec.* 17.

The President in the chair.

Hutches Traver, Esq. of Upper Harley Street, was elected a member of the Society.

The continuation of Mr. Webster's paper on the upper strata of the S. E. part of England, was read—

This part of Mr. Webster's paper begins by a description of the marine deposit which covers the lower freshwater formation in the Isle of Wight. The place where it may be studied to most advantage is Headen, near Alum bay. It here appears about half way up the cliff, is about 36 feet thick, and dips a few degrees to the north. The substance composing the principal part of the bed, is a pale greenish marl filled with shells, chiefly, cytherea, and oysters, in a very perfect state of preservation. The extensive stratum containing shells, which appears at Woolwich, and in many other parts of the London basin south of the Thames, are also considered by Mr. Webster as portions of the upper marine formation. Beds containing similar fossils occur in the Paris basin, covering the gypsum and gypseous marls of the lower freshwater formation.

The above strata in the Paris basin are covered by very extensive and thick beds of a pure sand, sometimes loose, sometimes concreted: with which is also connected that peculiar and valuable mineral known by the name of *meuliere*, or *burr-stone*. In the Isle of Wight there is nothing to correspond with these important beds, except a thin layer of sand; but in the counties round London occurs in detached blocks, a very pure siliceous sandstone, called the *grey-weathers*, which has been largely employed in architecture, and which is conjectured by Mr. Webster to be of cotemporaneous origin with the French sandstone.

The

The *Upper Freshwater formation*, one of the most remarkable and best characterized of any of the English beds above the blue clay, is best seen at Headen, in the Isle of Wight. Its thickness is about 55 feet, and though not subdivided into distinct strata, it varies considerably in texture. Much of it consists of yellowish white marl, more or less indurated, but friable and crumbling by frost. Many of the shells imbedded in this stratum are quite entire, consisting of various species of lymnex planorbes, helices, and other freshwater shells. Over this bed is a stratum of clay, with small bivalve shells, covered by a bed of yellow clay without shells, which latter is covered by a bed of friable calcareons sandstone, also without shells. To this succeed other calcareous strata, with a few freshwater shells, varying much in compactness from that of chalk to porcellaneous limestone. This formation appears to have covered nearly all the northern half of the Isle of Wight.

In the Paris basin are strata corresponding with these, both in their general composition, and in the fossils which they contain, distinguished, however, by certain peculiar characters that are detailed by the author of this paper.

---

Mr. Singer will commence his Lectures on Experimental Philosophy, on Tuesday the 18th of January, and continue them on each succeeding Tuesday and Friday.

A sketch of the arrangement may be obtained of Mr. R. Triphook, Bookseller, 37, St. James's Street.

Dr. Clarke's and Mr. Clarke's Lectures on Midwifery, and the diseases of women and children.

Dr. Clarke and Mr. Clarke will begin their next course of Lectures on Midwifery and the diseases of women and children, on Monday, January 24th, 1814. The Lectures are read every morning at the house of Mr. Clarke, No. 10, Upper John Street, Golden Square, from a quarter past ten to a quarter past eleven, for the convenience of students attending the hospitals.

For further particulars apply to Dr. Clarke, No. 1, New Burlington Street, or to Mr. Clarke at the Lecture Room.

# TO THE PUBLIC.

FOURTEEN years are now upon their close since I began the Philosophical Journal, which has been continued, without interruption, through the original series of five quarto volumes, and a second series of thirty-six volumes in octavo. Soon after my commencement, the Philosophical Magazine was established by my friend Mr. Tilloch, who has now very ably conducted it to its forty-second volume. During this period the sciences and arts have made the most rapid progress. Numerous philosophical and mechanical instruments and machines have been invented and improved. The theory and practice of astronomy have been greatly advanced. New planets have been discovered, and the structure of the sun more clearly ascertained. The rays of light have been subjected to new experiments, which have demonstrated their separate and distinct powers of illuminating and of heating; and the wonderful property upon which the phenomena of the Iceland crystal depend, but which

is now shewn to be inherent in both kinds of rays, and universally operative in all the cases of reflection and refraction. Chemistry has a second time, within our own observation, become a new science by the discovery of the effects of galvanism on its processes, and the developement of elementary substances of higher simplicity than were before known,—of the definite proportions of component parts,—the laws of elective attractions and of crystalline forms, and even to a certain extent of the remoter causes of those laws and forms. The cultivators of the sciences, the directors of the operations of the arts, public institutions and societies, have become every year more numerous and rapid in their increase; at the same time that roads, bridges, canals, and other national works, as well as private undertakings, have been every where established.

Under such circumstances of national vigour, I have felt it to be a proud situation to act as the Journalist of our own improvements, along with those which have been made on the Continent. My labour has been remunerated, partly by income, and amply by the marks of public and private respect which have attended them.

I have now to announce a change in the manner of my scientific intercourse with the public. I trust it will be found that that change will be beneficial. Upon many occasions, my Correspondents have complained, that the same academical papers and articles of information have

appeared in my Journal and in that of Mr. Tilloch, and have requested me to consult with him upon the means of preventing the Philosophical World from receiving the same materials in both. Similar remonstrances have also been addressed to him. But it seemed impossible to afford a remedy in two distinct works; both equally engaged to present to the public every thing that might prove new, interesting, and valuable. At this season it was in my contemplation to have made some changes which might have rendered my work, in several respects, more useful. I conferred with Mr. Tilloch; and the result of our deliberations has been, that it would certainly be best that we should unite, and that the joint product of our exertions and our correspondence should be consolidated in one work,—affording all that has hitherto been considered as desirable in the plans and conduct of both, but free from the objection last stated, and perhaps one or two more, upon which it is needless to enlarge.

*The* PHILOSOPHICAL JOURNAL *will henceforth be discontinued, and the* PHILOSOPHICAL MAGAZINE *will be conducted by* W. NICHOLSON *and* A. TILLOCH, *in the same manner as it has always been carried on, but with every attention to improvement, which the joint exertions of the Editors, and the communications of their friends and correspondents, can afford.*

I would, in consequence, request a continuance of

the same intercourse and support from my readers, by their orders for the *Philosophical Magazine*,—which will most conveniently be given to their own booksellers. Communications may be addressed to the Editors of the Philosophical Magazine, Picket Place, Temple Bar.

*London, Dec.* 31, 1813.

G. Sidney, Printer,
Northumberland Street, Strand.